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Contents

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Science and Technology

(From the Month's News Releases)

Atom Models

New hand-finished atom models conform to the latest theories as to how metals combine with organic or inorganic groups and are designed so that the completed molecule can be measured with a meter stick and the size in Angstrom units determined. The scale is 100 million to 1.

Bender

Combined in a three-in-one model, a tool for making offsets and bends up to 180° in ⅜", ½", and ⅝" outside diameter copper, brass, steel, and other light-gauge tubing requires no vise or fixtures. The price is so low that even the hobbyist can afford it.

Bonder

A new product, according to its manufacturer, sticks to anything, toughens with age, and never becomes brittle. It will seal leaks in household plumbing, garden hose, rubber footwear, oil tanks, and fuel lines, will repair cracked and broken ornaments, battery cases, and will waterproof ignition systems.

Hand Protection

A plastic-coated work glove resists chemicals, does not get "tacky" at temperatures to which workers' hands are normally exposed, and remains flexible in low temperatures. Available in fully coated or ventilated-palm styles, smooth or rough finish, knit wrist, safety cuff, or gauntlet, for men or women.

Adjustable safety mitts are hand pads of heavy horsehide, especially useful for handling brick, tile, metal sheets, and other sharp-edged materials where there is risk of hand injury.

Publications Noted

Air Repair, Vol. 1, No. 2, November 1951. A quarterly magazine devoted to air purification, published by the Air Pollution and Smoke Prevention Association of America. Editor: Robert T. Griebling.

Bulletin of the Research Council of Israel, Vol. 1, No. 3, August 1951. Quarterly. Published in Jerusalem. (English.)

Journal of Rational Mechanics and Analysis, published by the Graduate Institute for Applied Mathematics, Indiana University, Vol. 1, No. 1, January 1952. $18.00 per year. English, French, German, and Italian. Edited by T. Y. Thomas and C. A. Truesdell.

Notiziario dell' Istituto Vaccinogeno Antitubercolare, Vol. 1, No. 2, April–June 1951. Milan. 1,000 lire per year.

Lore, Vol. 1, No. 4, Fall issue. Published quarterly by the Milwaukee Public Museum. Obtainable only through membership.

Revista Cientifica, Vol. 1, No. 2. Available on an exchange basis to universities, scientific societies, research institutions, industrial firms, and scientific publishers. Address Faculdade Nacional de Filosofia, Av. Presidente Antonio Carlos, 40–6°, Rio de Janeiro.

The Science Reports of the Research Institutes, Vols. 1, 2, No. 1, January 1951. Tohoku University, Sendai, Japan, in four series: A, Physics, Chemistry and Metallurgy; B, Technology; C, Medicine; and D, Agriculture. (English.)

Shikoku Acta Medica, Vol. 1, No. 1. Shikoku Medical Association, Tokushima, Japan. Bimonthly, English and Japanese.

Yokohama Medical Bulletin, Vol. 1, No. 1, October 1950. (English.) Yokohama University School of Medicine.

Better Sewage Disposal

A cleaner that may be introduced into a septic tank drainage system at any of several different points clears clogged and sluggish lines or tiles and allows water to be absorbed more readily into the soil by removing greases and fats. A combination of 12 chemicals, it reduces odors and neutralizes acid soils, but will not reduce natural soil alkalinity. It is not harmful to Orangeburg tile, ceramics, concrete, cast iron, crushed stone, and the like.

Muff

A muff-type carry-all of plastic film will protect hands, purse, and small packages from those April showers, and folds up into a small, compact package when not in use. A large flap at the back opens wide for inserting packages.

Repair Kit

Plastic film patches in clear, red, green, and yellow, resistant to moisture, mold, oil, and grease, may be used to repair shower curtains, inflatable toys, garment bags, or similar articles made of plastic. Packaged in a repair kit with a tube of specially compounded adhesive, the patches are very inexpensive.

Air Movers

A device employing compressed air or steam will move large volumes of air quickly and economically. Depending on the situation, it can serve as a blower or an exhauster. Rapid, safe, and convenient, it would be useful in oil refineries, chemical and steel plants, steamships, airplanes, and in mining operations.

For homes, 24- and 30-inch fans will move 2600 to 5745 cfm, can be installed with a screw driver, and are reversible in action. Adjustable steel panels permit installation in windows up to 36 inches wide, and both front and rear guards are fingerproof.

Address a post card to Science and Technology, 1515 Massachusetts Ave., N.W., Washington 5, D. C., for further information about any item on pages iv and v.

Stop Shivering

An electric automatic control that may be applied to any thermostat turns heat down at night and up in the morning and is powered by a Telechron timing mechanism. It requires no alteration in present thermostats and is less expensive than a clock device.

Day of the Robot

By linking together an electronic counter and a mass spectrometer, petroleum products may be analyzed in one tenth the usual time. A mere trace of gas or liquid may be analyzed and the complete chemical composition typed on a sheet of paper in a matter of minutes. The instrument, which has been named "Miniac," can also be used on many other problems requiring complex, high-speed calculations. Engineers who developed Miniac have completed pilot models of Sadic, a relatively low-speed analog-to-digital converter, and Millisadic, which operates at high speeds, but doesn't bother with many decimal places. To achieve even more phenomenal speeds, the instruments can be combined.

Knob and Lock

A combination doorknob and lock features a Yale cylinder and converts any lock or latch into a tamper-proof lock. Installation involves merely unscrewing the present knob and replacement with the new one. Available at $3.50 in solid brass or in blue-white chrome with two matching keys.

Scallops Investigated

Drifting over Nantucket Shoals on the research vessel *Caryn,* of the Woods Hole Oceanographic Institution, a photographer and a shellfish biologist lowered a repeating underwater camera over the side and obtained an extensive view of two miles of ocean bottom. Although many of the photographs were out of focus because of the roll of the ship, they gave valuable information on the sea scallop. The work is part of a shellfish resources program supported by funds from the Massachusetts Division of Marine Fisheries and is a departure from the usual methods in that a normal population is being studied so that, if the need for restrictions ever arises, pertinent facts will be available.

Weather Buoy

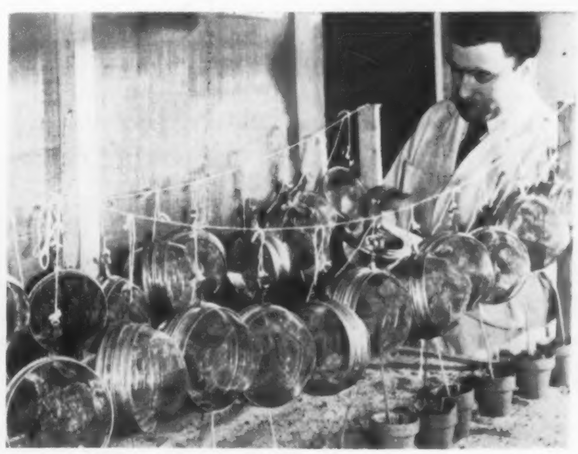
A free-floating buoy-type weather station transmits data by radio automatically for 30 days, at a predetermined sequence in 3-hour intervals, over distances up to 400 miles. Air and water temperatures, air pressure, and wind speed and direction are reported.

Pest Control

In a new research project, Bureau of Entomology and Plant Quarantine entomologists will cooperate with scientists in agricultural experiment stations in Kansas, Oklahoma, and Texas in studying the possibilities of reducing populations of small grain pests by cropping practices and chemical controls. Headquarters are at Stillwater, Oklahoma. The same bureau has proved that early spraying of grasshoppers at daytime temperatures ranging from 80° to 90° costs less and gives better control. Best time to catch them young may vary from May and early June in Texas to July, August, and part of September in North Dakota. Dieldrin, aldrin, and taxophene were the insecticides used.

At the University of Wisconsin, entomologists have discovered that chlordane, applied at 10 pounds per acre, seems to kill crab grass as well as insects. Two years after spraying the pest had not come back and other lawn grasses had not been injured.

A portable insecticide fog generator that has been in the experimental stage for nearly three years is now in production. The manufacturers claim that if used with any oil-base insecticide, it will effectively control all common insect pests. Powered by a 1½-hp gasoline engine, it is mounted on two 10″ × 1¼″ rubber-tired wheels and has a capacity of 4½ gallons.



In the photographs above scientists at the Boyce-Thompson Institute test new insecticides in the continual battle against insect pests. At the left cockroaches (under small screens) have been sprayed with allyl cinerin, and at right Mexican bean beetles have been allowed to feed on bean plants sprayed with the same chemical.

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Day Length, Migration, and Breeding Cycles in Birds*

ALBERT WOLFSON

Albert Wolfson has been experimenting with day length, bird migration, and the reproductive physiology of birds since 1938. Two years after taking his Ph.D. at the University of California in 1942 he went to Northwestern University, where he has been in the Department of Biological Sciences ever since. The work on which his article is based began there in 1946. Mr. Wolfson is secretary of the American Ornithologists' Union and has been chairman of their Committee on Research for the past two years.

EXPERIMENTATION has demonstrated that increasing day length is an important factor in initiating migratory behavior and gonadal growth in North Temperate species. By subjecting juncos (*Junco hyemalis* and *J. oreganus*) and crows (*Corvus brachyrhynchos*) to artificial increases in day length, it has been possible to induce a spring migration in midwinter.[1–3] Similarly, gonadal growth, including ovulation in some forms, has been induced out of season. On the basis of these findings, a number of theories have been postulated to explain the timing of spring migration and breeding under natural conditions. With respect to migration, it is held that the increasing day lengths of winter and spring induce a change in physiological state that ultimately initiates migratory behavior. A diagnostic feature of the premigratory state is a heavy deposition of fat. With respect to gonadal cycles, increasing day lengths are held to be stimulating, and decreasing, or short day lengths, inhibitory. Hence, in Temperate Zone species breeding occurs primarily in spring and summer.

Critical weaknesses in the theories presented above are the following: (1) Many species that migrate into North Temperate latitudes to breed winter in the equatorial region or in South Temperate latitudes. After December 21 birds in northern latitudes experience gradually increasing day lengths from an originally short day. However, birds in the tropics, especially near the equator, experience relatively constant day lengths of about 12 hours. Birds in the South Temperate latitudes experience gradually decreasing day lengths from an originally long day. Despite such diverse conditions of day length, migration is initiated at the proper time. (2) Breeding cycles occur in the tropics and on the equator where little or practically no change in day length occurs. To explain these

* The research reported in this paper has been supported by research grants from the Graduate School of Northwestern University. I am indebted to Roland K. Meyer and F. Greeley for permission to use unpublished data.

facts it has been suggested that factors other than, or in addition to, day length are important. For example, the occurrence of an internal rhythm largely independent of environmental factors, or of an inherent refractory period following the breeding season during which the birds do not respond to day length, has been postulated.[4,5] These interpretations are valid, and there is experimental evidence for both. If day length is important in North Temperate latitudes, however, it seems reasonable to assume that it is important also in the tropics and in South Temperate latitudes.

The results of numerous experiments in our laboratory lend support to another interpretation of the relation of day length to migratory and breeding periodicity. According to this interpretation, day length may be looked upon as a primary regulatory factor of migratory and breeding cycles at all latitudes. The purpose of this paper is to report briefly on the results of these experiments and their implications. The current intense interest in this problem,[4-8] but especially the tendency to minimize or reject day length as a regulatory factor, has prompted the publication of this synthesis. (Detailed reports of the experiments will be published later elsewhere.)

During the course of experiments in which the premigratory physiological state was induced in winter in juncos and other fringillids, a few individuals were subjected suddenly to constant long day lengths. When compared with individuals receiving gradual increases in day length it was noted that these birds responded sooner. On the basis of this observation it seemed possible that the total amount of light that a bird receives within a given period might determine the time of response; hence it was postulated that summation of day lengths, rather than increasing day lengths as such, might be the critical environmental factor in determining the time of migration.[9] If this were true, then the constant or decreasing day lengths to which equatorial and transequatorial migrants are subjected could still play a role in the timing of spring migration. Explaining the periodicity and timing of breeding in the tropics also seemed possible by means of such a concept.

Total light, or the cumulative action of daily photoperiods, has been considered in relation to the testis cycle of nonmigratory starlings (*Sturnus vulgaris*)[10-12] and English sparrows (*Passer domesticus*)[13] and the molt cycle in tropical weaver finches (*Euplectes, Vidua, Steganura*).[14] The experimental results point clearly to the importance of the daily photoperiod and the existence of threshold values for stimulation and inhibition. Total light was not effective in the starling or English sparrow when the daily photoperiod remained below the threshold value.

Summation of Day Lengths in Nature

Before undertaking experimental work to test the hypothesis of summation in relation to migration, a study was made of the summation of day lengths at different latitudes. Experimental work notwithstanding, conditions in nature could well preclude the possibility that summation is a regulatory factor. Some of the results of this study are summarized in Tables 1–3. The data indicate that there are only small differences in the summations (beginning December 21) as the time for spring migration approaches. For example, by March 15

TABLE 1

SUMMATION OF DAY LENGTHS IN NORTHERN LATITUDES BEGINNING DECEMBER 21*

Date	Latitude					
	0°	10°	20°	30°	40°	50°
Jan. 15	20,081 —	19,231 – 4.2%	18,357 – 8.6%	17,399 – 13.4%	16,300 – 18.8%	14,824 – 26.2%
Feb. 15	43,967 —	42,362 – 3.7%	40,742 – 7.3%	38,991 – 11.3%	36,965 – 15.9%	34,311 – 22.0%
Mar. 15	66,259 —	64,357 – 2.9%	62,483 – 5.7%	60,498 – 8.7%	58,243 – 12.1%	55,331 – 16.5%
Apr. 15	90,084 —	88,375 – 1.9%	86,762 – 3.7%	85,146 – 5.5%	83,396 – 7.4%	81,244 – 9.8%
May 15	113,177 —	112,146 – 0.9%	111,297 – 1.7%	110,628 – 2.3%	110,115 – 2.7%	109,807 – 3.0%
June 1	126,286 —	125,806 – 0.4%	125,561 – 0.6%	125,639 – 0.5%	126,114 – 0.1%	127,318 + 0.8%

* Figures are given in minutes and include civil twilight, since many birds are known to wake and roost in relation to civil twilight. Percentage indicates comparison with equator. Data obtained from *Supplement to the American Ephemeris* (1946).

TABLE 2

SUMMATION OF DAY LENGTHS IN SOUTHERN LATITUDES BEGINNING DECEMBER 21*

DATE	LATITUDE					
	0°	10°	20°	30°	40°	50°
Jan. 15	20,081	20,989	22,024	23,275	24,959	27,575
	—	+4.5	+9.7	+15.9	+24.3	+37.3
Feb. 15	43,967	45,713	47,689	50,106	53,316	58,237
	—	+4.0	+8.5	+14.0	+21.3	+32.4
Mar. 15	66,259	68,373	70,778	73,749	77,699	83,702
	—	+3.2	+6.8	+11.3	+17.3	+26.3
Apr. 15	90,084	92,063	94,388	97,333	101,293	107,388
	—	+2.2	+4.8	+8.0	+12.4	+19.2
May 15	113,177	114,569	116,318	118,660	121,949	127,192
	—	+1.2	+2.8	+4.8	+7.8	+12.4
June 1	126,286	127,179	128,432	130,234	132,884	137,295
	—	+0.7	+1.7	+3.1	+5.2	+8.7

* Figures are given in minutes and include civil twilight, since many birds are known to wake and roost in relation to civil twilight. Percentage indicates comparison with equator. Data obtained from *Supplement to the American Ephemeris* (1946).

individuals wintering at 30° south latitude would have had only 11.3 per cent more daylight than those wintering at the equator; those wintering at 30° north latitude would have had only 8.7 per cent less. At 20° north latitude the summation by April 1 is only 9.8 per cent less than at 20° south latitude. These differences in summation, which would be equivalent in time to approximately 10 days or less, seem negligible when compared with the differences in duration and changes in day length at these same latitudes. At 30° south latitude the day length *decreases* gradually from approximately 15 to 13 hours; at 30° north latitude it *increases* gradually from approximately 11 hours to 13 hours; on and near the equator the day length is relatively *constant* at approximately 13 hours. (Civil twilight is included in all figures.)

A detailed analysis of the precise relation between onset of migration and summation of day lengths remains to be made for different species and for individuals of the same species, but the results of this preliminary analysis of conditions in nature were regarded as sufficiently consonant with the working hypothesis to stimulate experimentation and further observation.

Experimental Analysis of the Role of Day Length

Experiments have been performed during the past five years using primarily the slate-colored junco (*J. hyemalis*) and the white-throated sparrow (*Zonotrichia albicollis*). Both these Temperate Zone species migrate through Evanston, Illinois, in spring and fall, but Evanston is also within the winter range of the junco. Several hundred individuals were used and, as a rule, observations extended over many months. In some cases, individuals were held under experimental conditions for two years. Observations were made on reproductive organs, body weight, fat deposition, and annual molt. In effect, the entire annual cycle was studied in relation to length of day. No attempt has been made to differentiate between duration of light, duration of darkness, or proportion of light to darkness as the effective stimulus. Hence, the terms length of day or photoperiod are used only relatively in this paper.

If summation is a factor, then wintering birds subjected to different daily doses of day length should respond in a definite sequence, with those receiving the largest daily amount responding first. To test this prediction birds were captured during the fall migration and were subjected to the following constant day lengths beginning December 4: 9, 12, 15½, 20, and 24 hours.[15,16] One group was continued under natural conditions. All groups showed an excellent fat response, indicative of the change in physiological state that precedes the onset of migration, and in a definite sequence. The 24-, 20-, and 15½-hour groups responded first, approximately 40 days after the start of the experiment. The 12-hour group was next, responding in approximately 80 days. The group under natural conditions, which had a mean day length of about 10 hours, responded in approximately 120 days. The 9-hour group responded in approximately 160 days. The response of the testis showed a similar sequence with one exception: the 15½-hour group responded a little later than the 20- and 24-hour groups. Thus, the fat response was initiated at four different times, the testis response at five different times, and the sequence of occurrence was directly correlated with the day

length to which the birds were exposed. The 20- and 24-hour groups showed marked testicular growth beginning in December, the 15½-hour group in January, the 12-hour group in March and April, the natural group in April and May, and the 9-hour group in November, almost one year after the start of the experiment. With respect to the annual molt, it began first in the 15½-, 20-, and 24-hour groups, and much later in the natural group. Molt did not occur in the 9- and 12-hour groups. It is noteworthy that the following events in the annual cycle—premigratory fat deposition, formation of gametes, gonadal regression, and initiation of annual molt—which under normal conditions occur in approximately 240 days—occurred in approximately 95 days in the 15½-, 20-, and 24-hour groups.

The present interpretation of these results is as follows. The daily photoperiod, whether constant or gradually increasing, induces an increment of physiological change. The magnitude of the increment is proportional to the daily photoperiod, and it appears to be near maximum at 16 hours daily. Its minimum is not known, but it occurs at 9 hours per day, or less, for the gonadal and fat responses, and at more than 12 hours per day for the molt response. The summation of the increments eventually reaches a threshold, at which time the various responses are manifested. During the period of summation (or the induction period) prior to the initiation of the response, no marked manifestations of the daily increments are evident in fat deposition, gonadal growth, or molt. The thresholds and daily increments for the three responses are apparently different and are somewhat independent.

Since the length of the photoperiod determined the time of initiation of the various responses, it seemed likely that it would influence the rate and degree of the responses once they have begun. Some relevant data were available from the previous series of experiments, but a new series was designed specifically to answer this question. Birds were caught during the spring migration in April and May and subjected to constant day lengths of 9, 12, 20, and 24 hours, and to natural day lengths. The gonads in these birds were partly developed initially, and they also showed heavy fat deposits, indicative of the migratory physiological state.

The results of these experiments were generally in accord with those of the previous series, but a number of remarkable differences were found. The longer photoperiods of 20 and 24 hours induced a more rapid and a greater development of the gonads compared with the birds under natural conditions and 12 hours. This was especially true in some of the females. In the 9-hour group the gonads regressed almost immediately. In the previous experiment it was shown that a gonadal response could be induced with a constant 9-hour photoperiod, yet in this case regression occurred. In the present experiment the testes were in an advanced condition correlated with the natural day lengths of 14 hours or more at the start of the experiment. When the day length is reduced to 9 hours, perhaps the daily increment of gonadal stimulus is too small to meet the demands of the partly enlarged testis. This interpretation is suggested also by the fact that these birds will initiate another gonadal cycle many months later if they are held at the 9-hour photoperiod. Whether complete spermatogenesis will occur in this case remains to be determined, however.

Is the reduction in day length as such responsible for the regression in the 9-hour group? Probably not, for in the 12-hour group, which experienced a reduction in day length (from natural conditions) of approximately 2 hours at the start of the experiment, gonadal growth continued, but at a slower rate apparently than in the natural group. The extent of development shows no marked differences, but one unexpected and highly significant difference does occur. At the present date (January 2, 1952) most of the male juncos subjected to the 12-hour photoperiod beginning April 6, 1951 (but not any of the white-throated sparrows), are still in breeding condition. In the natural, 20-, and 24-hour groups the gonads regressed (with one exception) after several months of activity. In the

TABLE 3

PERCENTAGE DIFFERENCES IN SUMMATION OF DAY LENGTH BETWEEN NORTHERN AND SOUTHERN LATITUDES BEGINNING DECEMBER 21*

	Date	10° N	20° N	30° N	40° N
10° S	Mar. 1	− 6.6	− 9.8	− 13.1	− 17.0
	Apr. 1	− 4.9	− 7.1	− 9.4	− 11.9
	May 1	− 3.0	− 4.2	− 5.3	− 6.5
	June 1	− 1.1	− 1.3	− 1.2	− 0.8
20° S	Mar. 1	− 10.2	− 13.2	− 16.4	− 20.1
	Apr. 1	− 7.6	− 9.8	− 12.0	− 14.5
	May 1	− 4.9	− 6.1	− 7.2	− 8.3
	June 1	− 2.0	− 2.2	− 2.2	− 1.8
30° S	Mar. 1	− 14.1	− 17.0	− 20.1	− 23.7
	Apr. 1	− 10.9	− 12.9	− 15.1	− 17.5
	May 1	− 7.2	− 8.4	− 9.5	− 10.6
	June 1	− 3.4	− 3.6	− 3.5	− 3.2
40° S	Mar. 1	− 18.9	− 21.6	− 24.6	− 27.9
	Apr. 1	− 14.8	− 16.8	− 18.9	− 21.1
	May 1	− 10.3	− 11.4	− 12.4	− 13.5
	June 1	− 5.3	− 5.5	− 5.5	− 5.1

* Percentage difference is given in terms of southern latitudes throughout the table.

12-hour group the activity of the testes has been maintained so far for almost eight months. Fat deposition continued in all the groups, but the fat deposits disappeared first in the 9-hour group. The largest fat deposits were seen in the 12-hour group. The deposits disappeared (with one exception) in the natural, 20-, and 24-hour groups before they molted, but most of the birds in the 12-hour group, including some that are not in breeding condition, have not yet lost their fat. In this group and in the 9-hour group, no molt has occurred.

The results of the previous experiments showed that the daily photoperiod determined the time at which the response begins. The results of this series demonstrate that the daily photoperiod regulates also the rate at which the response proceeds, the degree of response, and the time when the activity phase of the cycle ceases. Some data suggest that the extent of regression is also influenced by day length, and some independence of the three types of response—fat, gonadal, and molt—was again demonstrated.

The observations on gonadal growth correlate well with the fact that birds that migrate from the tropics or southern latitudes to high northern latitudes to breed arrive on their breeding grounds ready to breed, and have relatively short breeding periods. Certainly other factors are involved in determining actual breeding, but the potentiality to breed could continue to be timed by the day length conditions which the birds encounter while on migration.

Previous studies have shown that gonadal activity cannot be maintained indefinitely in Temperate Zone species.[4] Regression occurs spontaneously after a period of activity, in spite of long, stimulating photoperiods. Moreover, following this regression there is a period of time during which light cannot induce gonadal activity. This period has been called the refractory period. By subjecting birds to increasing photoperiods in the fall it has been found that the refractory period in a number of species ends somewhere between the latter part of October and late November. The fact that male juncos subjected to different photoperiods remained sexually active for different periods of time raised the question of the role of day length in the occurrence and duration of the refractory period. A number of experiments have been performed. The first series was designed to answer two questions: (1) Could a highly stimulating day length, such as 20 hours, induce a fat and gonadal response beginning in October and early November, during the presumed refractory period? Heretofore, gradually increasing day lengths or constant day lengths of shorter duration had been used. (2) What would be the effect of a treatment on short days before subjecting the birds to 20 hours of day length?

Juncos and white-throated sparrows were captured during the fall migration (October through early November) and subjected immediately to 20 hours of day length. Several small groups were pretreated for one or two weeks with an 8-hour photoperiod. The experiment ran until May of the following year. The results of this experiment showed that most of the birds could not be stimulated by 20-hour days starting in late October or early November. Some responded after eight weeks, but some showed no response as late as six months after the start of the experiment. This result was surprising, since juncos subjected to 20-hour days beginning December 4 showed excellent gonadal growth five weeks later. In the groups that were pretreated with 8 hour photoperiods, more of the birds responded. Lack of response after six months or more of exposure to stimulating photoperiods beginning in October and early November has also been demonstrated in the golden-crowned sparrow (*Z. coronata*).[17, 18]

The results of this first series of experiments were interpreted as indicating the possibility that the refractory period—as an inherent period during which birds are not responsive to light—does not exist. It seemed, rather, that the occurrence of this period is related to day length, and that when it occurs short days are required before this phase of the cycle can end. Instead of a cycle with an inherent refractory phase which is not responsive to light, we could have a cycle that is dependent on, or regulated by, light in both its phases. Under certain photoperiodic conditions, therefore, there will be no alternation of active and inactive periods, but only the continuance of either phase.

The second series of experiments was designed to test this interpretation. Juncos were captured in the spring and were subjected to 20 hours of day length to speed them through their sexual development and annual molt. In the latter stages of their molt in mid-July and after their gonads had regressed, their day length was suddenly reduced to 9 or 12 hours. Six weeks later (August 27) they were exposed to 20 hours again. On this same date, a group of birds that had been held under natural conditions of day length and had gone through their sexual phase and most of their annual molt was exposed to 20 hours. The difference between this group and the others was that while the others were receiving 9 or 12 hours of constant day length for six weeks, this group was

experiencing gradually decreasing day lengths from approximately 16 hours (including civil twilight) to 14½ hours on August 27. The results of this experiment showed that the birds subjected to reduced day lengths of 9 and 12 hours responded to 20-hour days. The birds retained under the 20-hour photoperiod, and the birds exposed previously to natural day lengths, remained in the quiescent phase. A variation of this experiment was performed using white-throated sparrows that were under natural day-length conditions prior to treatment on short days. The results were confirmatory in part. Only the birds that were treated with a 9-hour photoperiod for five weeks before exposure to 20-hour photoperiods responded well. The groups that were treated with 12-hour and natural photoperiods did not respond.

From the results of these two series of experiments it seems likely that there is not an inherent refractory period during which birds are unresponsive to light, but rather a period of regression and inactivity which is induced by stimulating day lengths and during which short photoperiods are required before recovery can occur and a period of activity ensue. Without short photoperiods the metabolic change necessary to permit the recurrence of fat deposition and gonadal growth may not occur (or is delayed for a long time). The data suggest also that the rate and degree to which this change occurs are regulated by the length of day, the shorter day lengths permitting a more rapid change and a stronger response to the stimulating day lengths that follow. Further studies are planned to examine this point and other problems raised by these series of experiments.

When the results of all the experiments are considered in relation to possible regulation of migratory and reproductive rhythmicity, it is evident that the length of day may be an important factor in many ways. If we begin with the period of sexual quiescence that follows the breeding period, the available data indicate, for the species studied:

1) That short days must occur if the cycle is to continue in phase with the seasons. If short days occur and remain constant, the growth phase of the cycle will appear, but more slowly. Long days inhibit the occurrence of the growth phase. The length and duration of the short days appear to regulate the duration and extent of the quiescent phase, and the degree of response of the growth phase that follows.

2) When the growth phase begins, the rate at which the gonads respond and the time of occurrence of fat deposition are functions of the length of day, being extremely rapid under long days and extremely slow under short days.

3) The duration of the response—that is, the length of time the gonads are in breeding condition, or the fat deposits remain, or the plumage is retained—is also determined by the length of day. The longer the photoperiod, the shorter the period of maximum response; when day and night are equal, the growth phase and maximum activity are maintained for longer periods of time.

4) The occurrence of regression and the quiescent phase appears to be an inherent property of the organism, but the time at which it occurs seems to be governed by the photoperiod. This is based on the assumption that the juncos that are still producing sperm after 8 months, and still show fat deposits, will eventually show regression and loss of fat. If they do not, then the occurrence of regression is governed also by the photoperiod. Once the regressive phase begins, its duration, and possibly its extent, are regulated again by the photoperiod.

In summary, experimental results indicate that the length of day governs the time of occurrence, rate of development, amplitude, and duration of the phases of the gonadal, fat, and molt cycles. Our results confirm in large measure, and amplify, the excellent studies of Burger on the starling, and support his general conclusion that "day lengths affect the whole seasonal cycle, and do not merely fire an internal mechanism which, once set off, is self-controlling."[4] Further data are needed to determine whether the *occurrence* of regression and the *initiation* of gametogenesis are independent of external factors. The available data from our experiments are inconclusive on these important points. If the occurrence of these alternating processes is shown to be an inherent property of the organism, as seems likely, then the periodicity may be spoken of as an inherent rhythm. The term rhythm in this case, however, would refer only to the *alternation of these processes* and could not be construed as implying the time of occurrence, rate of development, duration, or amplitude of the two phases. All these aspects appear to be under the direct control of day length and are undoubtedly modifiable by other biotic and environmental factors. Another weakness in our present interpretation is that it is given only in terms of day length and the manifest response. Although the gonad may not exhibit a marked response, the pituitary, which regulates it and which is known to respond to light, could be responding. Before the precise relation between day length and the physiology of the organism can be stated in terms of excitation or inhibition, or the rate and degree of response, careful studies of the pituitary, thyroid, and adrenal glands must be made in which the synthesis and the secretion of hormones are differentiated as separate responses. In this connection, a study of the gonadotrophic activity of the dried pituitaries of cock pheasants by Meyer and Greeley (unpublished data) shows a periodicity of gonadotrophic activity which cor-

relates well, but not completely, with the testis cycle.

Day Length, Migration, and Breeding Cycles

On the basis of the experimental results and the interpretations presented here, the timing of spring migratory behavior in migrants wintering at the equator, or in the Southern Hemisphere, and the regulation of breeding cycles in the tropics—the two critical problems that weakened previous theories—can be explained.

With respect to migration, experimental work must be done with equatorial and transequatorial migrants before the explanation given above can be accepted, but until that is done observations can be made which have a bearing on the interpretations presented. For example, if birds at all latitudes begin their spring migration in response to generally similar summations of day length, then they should start with approximately the same physiological state. A limited number of observations on the gonads and fat deposition indicates that this may be true for some species.[1, 2, 19–24] If they do start with the same physiological state, then migrants passing through a station in the North Temperate latitudes should show different states of sexual development. Those in most advanced condition should come from farthest south, assuming general agreement in rate of growth of testes and speed of flight. Furthermore, birds that fly long distances should arrive on their breeding grounds with their gonads in breeding condition. Collections made at Evanston during spring migration suggest that this may be true. It was found that some of the species which winter in the central southern United States and arrive in Evanston in April—e.g., slate-colored junco, white-throated sparrow, swamp sparrow (*Melospiza georgiana georgiana*), field sparrow (*Spizella pusilla pusilla*), etc.—had only partly developed testes, whereas some of the species arriving in May and known to winter farther south—e.g., gray-cheeked thrush (*Hylocichla minima minima*), olive-backed thrush (*Hylocichla ustulata swainsoni*), northern yellow-throat (*Geothlypis trichas brachidactyla*), etc.—had gonads in breeding condition. Differences in species and subspecies are to be expected, and individuals of the same species wintering under identical conditions may also show some variation.[2, 20–22] This is a matter that requires further study, but it is one that can be studied readily under natural conditions.

The problem of breeding seasons in the tropics cannot be explained simply by any one factor in the environment. Moreover, it is not possible to extrapolate directly from experimental studies of the gonadal cycle to breeding cycles in nature, especially when the species used are North Temperate and the problem is one of breeding cycles in the tropics. Nevertheless, it seems desirable to examine the problem in relation to the hypothesis presented. The basic premise in the discussion that follows is that breeding is preceded by gametogenesis; hence, what controls gametogenesis has a fundamental role in the regulation of breeding seasons. It is understood, however, that after gametogenesis occurs, or is initiated, other factors come into play to regulate the time and duration of breeding. A sharp distinction must be drawn between the potentiality to breed, which is what the experimentalist analyzes, and the breeding season, which is analyzed by the naturalist.

Studies of breeding cycles at all latitudes led Baker[25] to the conclusion that "the main proximate causes of the breeding seasons of birds in nature are thought to be temperature and the length of day in the boreal and temperate zones, and rain and/or intensity of insolation near the equator." More recently, a re-examination of the problem has led to the same general conclusion that day length and temperature in the higher latitudes, and humidity and rainfall in the tropics, are correlated with the breeding seasons.[7, 26–31] The significance of these climatic factors is believed to lie in their effect on food supply. Breeding seasons are regarded as an adaptation and occur apparently when the young can be reared at the time of maximum food supply. If the breeding season is adapted to environmental conditions operating toward its close other factors must be postulated for the initiation of the cycle. Although day length and temperature are acceptable for higher latitudes, in the tropics both "are too nearly constant to offer a possible explanation." [7]

There is little doubt that the experimental studies which employed increasing and decreasing day lengths to alter the gonadal cycle, and the fact that day length and temperature vary periodically in higher latitudes, have played a large part in the derivation of the above conclusions. Our experimental findings suggest that day length should not be ruled out as a regulatory factor in the tropics simply because it is relatively constant. The pertinent question is whether tropical species respond to day length. Experimental studies with tropical species are urgently needed to answer this question, but the data from two series of experiments[14, 32] and other observations suggest that the gonadal and molt cycles of at least some

tropical and equatorial birds can be altered by changes in day length.

Experimental studies with tropical species were performed in the Chicago area, using whydahs and weavers (*Steganura, Euplectes,* and *Vidua*) that were imported from Africa. These studies demonstrated clearly that the daily photoperiod was a fundamental factor in determining the time of molt, the type of plumage, and probably the activity of the gonads.

Observations on the breeding season of birds transported from the tropics or the Southern Hemisphere to northern latitudes[33] show that some tropical species change the time of their breeding season to correspond with the seasons in the Northern Hemisphere. For the tropical species that do not change it has been suggested that there is an internal rhythm not readily altered by environmental conditions.[33] Another possible explanation is that the day length requirements of these species are such that the time of the breeding season in relation to the calendar year may not be altered. The most detailed observations on the effect of transport on a tropical species are those of Orr.[34] These are especially significant because the species used, the Galápagos finches (*Geospiza* sp.), are restricted to the Galápagos Islands. The individuals transported were taken within 60 miles of the equator and shipped to the San Francisco area, where they arrived in April. The normal breeding period in the Galápagos Islands extends from mid-December to April and is correlated with the rainy season, but breeding, to a limited extent, may occur in almost every month of the year. The captive birds in San Francisco bred from March to November inclusive, but the nesting period for most of the captive birds was confined to spring and summer. In this case, as in others cited by Baker, it is probable that day length is the factor responsible for the change in breeding season. On the basis of our experimental results it may be postulated that the gonadal cycle in these species requires approximately a 12-hour photoperiod (or a particular summation) to achieve breeding condition. A longer photoperiod may or may not accelerate the cycle, but when a shorter one occurs the gonads either grow slowly or only partly and do not achieve breeding condition. Hence, in the San Francisco region the birds bred essentially from the spring to the fall equinox, when the day lengths are 12 hours or longer. Taking civil twilight into account, the period would be extended by several weeks at both equinoxes. During the three months when they did not breed at San Francisco (December, January, and February) the day lengths (including civil twilight) varied from approximately $10\frac{1}{2}$ hours to 12 hours on February 28. Other explanations of the relationship between day length and breeding in these species are possible. Experimental work must be done before the precise relationship can be determined, but the present data suggest that some relationship exists. Observations on the molt cycle also suggest that it was influenced by day length. The first-year plumage is generally replaced completely between February and June the year following that in which the young are hatched. Thereafter, only one molt occurs annually at approximately the same time. All the birds that arrived in San Francisco in April 1939 had just completed their annual molt or were still molting. Some of these were young of the year. In the latter part of August and during September of the *same* year all the birds that survived molted again. This was 5 to 6 months earlier than the annual molt would have occurred on the Galápagos Islands. In succeeding years, these same birds had only one annual molt, which took place usually between the middle of August and the early part of October. Similarly, the molts of some of the young hatched during the summer and autumn of 1940, the year following the arrival of the parent stock, did not conform with those of young hatched in succeeding years. Instead of molting for the first time when they were a year old, they molted in the spring when they were approximately six months old. In the fall of the same year they molted again. Thus, when they were slightly over a year old they were in a plumage corresponding to that of the normal third year. Following this, however, these same birds reverted to the normal rhythm of but one complete molt a year. Finally, the normal seasonal change in bill color, which is correlated with the breeding season, was not observed in the San Francisco birds. The black color associated with the breeding season occurred permanently in most of the birds.

A number of other correlations point to a relationship between day length and reproduction in the tropics. (1) In many tropical species which occur on both sides of the equator the breeding periods in the northern and southern populations are correlated with the seasons and hence occur at opposite times of the year.[25, 35] Frequently this is associated with the wet or dry season, but often it is not. (2) In many groups of birds, and even in a species with wide distribution, clutch size tends to be smaller in the tropics than in the temperate latitudes.[36] Lack's interpretation of this observation is that the longer day enables parents to find more food per day and hence raise more young at one

time. As an ultimate factor affecting clutch size, there is little doubt that food supply is important. Our interpretation, however, is concerned with day length as a proximate factor which affects the reproductive physiology of the bird. For example, in the domestic fowl, egg laying (which is not homologous to clutch size) is greatly influenced by latitude.[37, 38] As a rule, maximum production occurs in spring and summer, and the minimum in fall and winter. The peak of egg laying in New Zealand (approx. 40° S. latitude), therefore, occurs in November, and in the central part of the United States (Connecticut, approx. 40° N. latitude) in May. At the limits of the tropics (approx. 23° north and south of the equator) egg laying varies with the seasons, as in the Temperate Zones. But near the equator, for example, in southern India (approx. 10° N. latitude), egg production is nearly uniform throughout the year. It is interesting to note that the rate of production (expressed as the percentage of egg production that would be possible if one egg were laid each day) is about 40 per cent in southern India, but varies from a maximum of about 60 per cent to a minimum of about 20 per cent in the temperate regions. (Above statements are based on hens with an annual egg production of about 150 eggs.) The environment in this case is not only influencing the rate of production, but it seems also to be responsible for the cyclical nature of egg production. The correlation between long days and short days and the maximum and minimum in the annual egg-laying cycle, and the fact that additional lighting in winter promotes greater egg production,[39] suggest that day length is responsible for the differences in egg laying in the tropical and Temperate Zones. In the turkey the number of eggs laid annually can be almost doubled by continuous light.[40]

(3) Related to the above observations on reproductive activity in the female is the observation that the maximum size of active testes in tropical species is only 8 to 67 times the size of minimum inactive testes, whereas in temperate species it ranges from 267 to 2096† times the minimum size.[41] Again, a more stimulating effect of longer day lengths is suggested.

The gonadal response in experimental juncos and white-throated sparrows simulates some of the correlations between latitude and reproductive activity. Birds subjected to 20-hour photoperiods achieve breeding condition rapidly, show a tendency toward larger and more active gonads, and exhibit maximal activity for only a short period. Birds held at 12-hour photoperiods achieve breeding condition at a slower rate, show a tendency toward smaller and less active gonads, and remain sexually active for long periods of time.

The relation between day length and the gonadal cycle in tropical species is not known, but the point that must be emphasized is that day length cannot be ruled out a priori as a fundamental regulator of migration and breeding cycles in the tropics simply because it is relatively constant. In a similar vein, day length cannot be ruled out at other latitudes because not all birds are breeding when the days are increasing, or because the gonads begin their growth phase when the day lengths are decreasing or are relatively constant. When day length is suggested as a regulatory factor at all latitudes, it is not meant to imply that all birds will react to it in precisely the same manner. Our experiments indicate that day length is concerned not only with the initiation of the growth phase of the testis, but also with the rate and extent of growth, the duration of maximum activity, the initiation, rapidity, and extent of regression, and the duration of the inactive phase. There is no reason to believe that all these aspects will be regulated by day length in precisely the same manner in all species. Plants, for example, have been classified according to their photoperiodic responses into short-day, long-day, day-neutral, and intermediate types. In short-day plants flowering is induced by relatively short photoperiods (10 hours or less); in long-day plants by long photoperiods (14 hours or more); in day-neutral plants by photoperiods 10 to 18 hours long, or by continuous illumination; in intermediate types flowering occurs when day lengths are 12–14 hours, but shorter or longer photoperiods inhibit flowering. Furthermore, photoperiodism is an important factor in the natural distribution of plants. Genera, species, and varieties have developed photoperiodic responses which synchronize flowering and seed production with particular day lengths.[42]

In birds, experimental studies and an analysis of breeding seasons in relation to latitude suggest that there are different responses to day length. In the experiments described above with the slate-colored junco, a North Temperate species, the annual molt did not occur when the birds were exposed to short photoperiods (9 and 12 hours), but it did occur under long photoperiods (16 and 20 hours, and in continuous light). In *Euplectes,* a tropical species, an individual exposed to 16-hour photoperiods beginning December 5 did not show

† This figure is for the slate-colored junco, and Rowan[1] is cited as the source. Our data for this species and related forms indicate an increase of less than 1000 times.

an annual molt, but retained the nuptial plumage for more than a year.[14] A control held under natural conditions of day length (approximately 9 hours) did undergo an annual molt. This difference in molt response (and probably also gonadal response) suggests that the annual cycle of the junco is adapted to a particular summation and/or to long days and that of *Euplectes* to a particular summation and/or short days. The experimental results correlate well with the latitudinal distribution of these forms and suggest that, as in plants, photoperiodism may be a factor in the distribution of birds. Other experimental results that demonstrate a difference in response to day length are the following: (1) Under identical experimental conditions of day length the testes of the resident race of the Oregon junco (*J. oreganus pinosus*) developed to breeding condition in approximately 50 days. In the same period the testes of the migrant races (*J. o. thurberi*, *J. o. shufeldti*, and *J. o. oreganus*) developed to only approximately one twentieth that size and contained only spermatocytes. In addition, the migrant races showed a heavy deposition of fat, whereas the resident race did not. Under natural conditions the same differences are found in this species and in others.[2, 20, 21] (2) When juncos and white-crowned sparrows (*Z. leucophrys leucophrys*) were subjected to 8- or 9-hour photoperiods in May when their gonads were in breeding condition, they showed gonadal regression immediately. English sparrows treated similarly did not.

The study of breeding seasons in relation to latitude shows that two basic patterns occur.[25] In one there is a tendency for birds to start their egg laying earlier and earlier as one proceeds from the temperate to the northern tropical and equatorial zones (for example, Accipitres, Coraciiformes, and to a lesser extent Passeres). In the other, there is a tendency for the birds to breed *later* as one proceeds from the temperate latitudes to the northern tropical and equatorial zones (for example, Charadriiformes, Grallae, Herodiones, and Anseres). These patterns suggest that the gonadal response to day length differs in these two groups. It remains for experimental work to determine whether this is true, and if it is, to work out the relation of the various aspects of the reproductive cycle to day length.

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On Game-Learning Machines

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THERE has recently been some interest in the possibility of designing a computing machine to play a game.[1,2] The problem discussed in this paper is a perhaps premature extension of these thoughts: Can one conceive of a machine that has absolutely no initial built-in knowledge but does have an "intelligent" ability to learn almost any game through experience alone? Obviously this must be possible to some extent, but it is proposed to examine the question[3] more thoroughly in the hope of determining how "intelligent" such a machine might be. For lack of a better method, "intelligence" will be tested by the machine's performance against a human opponent who, knowing its construction, could take advantage of any possible machinelike inadaptability or lack of imagination.*

It should be emphasized that the problem considered here differs fundamentally from that of designing a machine to play a good game of chess. Our present problem specifically states that no knowledge may be built in. We are interested in the extent to which a machine can *learn.* It is obvious that with such a limitation the machine cannot play a better game of ticktacktoe than one specifically designed to play ticktacktoe. But the limitation has advantages as well: The machine is not biased toward any particular game; it can, with no modification, be set to playing a simple variant of poker. The machine must learn for itself all that it will eventually "know," but it should, ideally, be capable of learning almost any type of game. The goals are *adaptability* first and proficiency second. Moreover, proficiency is to be judged more by the accumulation of winnings than by the methods the machine might use to obtain winnings. A human opponent might derive a certain type of satisfaction if he "fools" the machine into playing poorly by occasionally losing to it, but unless he actually gains greater winnings in this way, it is questionable just who is fooling whom.

The discussion that follows is to be regarded as little more than a list of suggestions. The methods of attack are far too intuitive (depending mainly on the author's foreseeing possibilities for the human opponent) to defend any claims of logically proved soundness in the final design. Similarly, it will be indicated that certain compromises are inherently required, but again the discussion cannot be said to prove this.

The problem requires the machine to start "life" with absolutely no built-in knowledge concerning the rules of the game or of the values of various situations. Nor can the type of game be restricted. A large memory will record various games as they are played, and this information alone can be used in developing methods of play. (Note in particular that the machine must "discover"† for itself the most elementary feature of ticktacktoe: that the value of a position is independent of the moves that precede it.) Possible moves may be coded by numbering; the machine is given the code number of the move chosen by its opponent and is told how many countermoves are now open to it. The number sequences corresponding to games played may be recorded in the memory along with the payment received at the end of each game. We shall assume that all circuits, including the memory, have sufficient capacity to

* It is not entirely clear that this criterion is completely fair. In a very real sense, the professional magician deceives his audience by using his knowledge of how their minds work.

† Throughout this paper a certain amount of anthropomorphic terminology is used. These words merely designate various aspects of the machine's external behavior without implying any important analogy to similar aspects of organisms. It would perhaps be better to use entirely new words, but this would make the discussion rather clumsy. The situation is much like that in multidimensional geometry, where such words as "sphere" or "angle" really have no meaning in themselves, but where the terms are often used without definitions in the interest of brevity.

handle all possible individual games of any one type that the machine may be asked to play. (An interesting but entirely different problem would result upon relaxing this requirement.)

The machine therefore plays a "normalized form" of any game. Each game is, for its purposes, reduced to one in which the players alternately choose a number until at some point, possibly dependent on the particular numbers chosen, the game ends with payment of the winnings. The payment function may be only statistically determined: This arises very simply if the game contains elements of chance; it may arise more indirectly if some information is hidden from one player—for example, the value of an opponent's poker hand. Finally, the statistical properties of the payment function may themselves change with time; for example, the expected return from bluffing in poker depends upon the opponent's strategy.

In games such as chess or ticktacktoe, how are relative values to be assigned to various games won? It would appear that the most disastrous losses would be the shortest, and that the most successful "wins" would also be the shortest. This suggests using as the payment function, F, not the actual winnings, f ($= \pm 1$, 0 in such games), but the winnings divided by the number of moves in the game: $F = f/n$. Indeed this is a realistic goal in any game: maximum return per unit of effort. (Any function monotonic in F would serve equally well.)

With these preliminaries, the central problem, the *modus operandi* of the machine, may now be discussed. Evidently the first game must be played at random. It might appear that any arbitrary system of exploration would serve as well, but this is not entirely true. Some games, such as matching pennies (where the players *choose* heads or tails) require concealment of intentions above all other considerations. Thus a provision for truly random play will be a valuable asset to the machine. (One possible mechanism for this could be based on the position of a constantly rotating wheel; the human opponent's relatively erratic speed of response would preserve the random element.) Note also that the machine must be presumed to play seriously as soon as it possesses a modicum of experience. A ruthless opponent would never allow the machine a complete view of all possible games, and it therefore cannot be expected to wait until its experience is in any sense complete.‡

Some method must be included for determining when the machine would do better by striking out at random rather than by following previously disastrous patterns. This may conveniently be done by assigning to untried moves a value F_0 to serve as a comparison reference in relation to the established F. The value F_0 would most naturally be set equal to the expected return for random play. This quantity in turn may be derived from the returns of games in which random play was used. Relatively simple methods for doing this will develop later.

Consider now the mid-game, fair-experience situation. Having learned the opponent's latest move, the machine must search its memory for games with the same initial train of moves; it will then examine the outcomes (F) of these games and in some manner utilize these F to select its next move. It might at first appear that the machine should, for each possible countermove, select the lowest F that has followed, and then choose that reply whose minimum F is greatest. If the game involves chance elements, however, the machine would then play by the unrealistic rule of minimizing its *possible* loss rather than minimizing its *average* loss. This observation suggests that the quantity to be maximized should be the *average* return-per-move $\overline{F}$, which has resulted from the choice of a given reply. Note further that, in "rational" games such as ticktacktoe, the opponent will do his best to make this average actually equal to the above minimum, so that the averaging process will not imply any vitally important loss of information even in such games.

The *modus operandi* of this preliminary design may then be summarized as follows: In each game, the machine keeps track of the partial sequence of numbers representing that part of the current game already played. In its memory is stored, under this partial-sequence indexing, possible countermoves with their associated averaged-return-per-move, $\overline{F}$; as-yet-untested moves are associated to a value F_0 described above.§ In selecting its reply, the machine chooses the move or moves with the largest $\overline{F}$ (including F_0 in the comparison) and, if this is not unique, selects (truly) at random among those of equal value.

It is futile to attempt any complete analysis of the effectiveness of this design with the vague methods now at our disposal. We may, however, use these methods to find faults in the design, even though it is difficult to estimate their "impor-

‡ But we do assume that the memory is large enough to accommodate a complete view of all possible games should it ever meet them all.

§ Note that each and every move in each and every possible situation is thus assigned an F—though most of these equal F_0 until a vast amount of experience has been acquired.

tance" (and even though it is impossible to be sure that all faults are thereby uncovered).

First note that, should an exceptionally skillful opponent suddenly take the place of a relatively mediocre player, the machine, basing its play on averages, will not at first be aware of the change; the losses must pile up sufficiently to affect the averaged values. This suggests that the averages should be computed in such a way as to give greater weight to more recent experiences. Indeed, the finite digit capacity of any machine requires that this be done, for, otherwise, current returns would eventually affect the averages by amounts too small for the machine to calculate. It is thus reasonable to stipulate that changes in an average shall eventually be computed as though the previous value were based on, say, N games irrespective of the actual number of such games played.

It would be even simpler to compute averages as though the number of previous games were *always* equal to N (even when they are actually fewer). This simplification even has some advantage: In the early "life" of the machine, the $\overline{F}$ will not be especially significant, and the machine should be encouraged to ignore information for a while until some modicum of experience has been gained. In conjunction with a feature to be discussed later, the assumption of a constant value for N accomplishes just this result while at the same time allowing extremely unfavorable moves to be rejected fairly quickly. We will thus suppose that the new $\overline{F}$ are always computed on the assumption that the old ones are based on exactly N games: $\overline{F}' = (N\overline{F} + f/n)/(N+1)$.

A rather more serious weakness in the proposed design appears upon more specific consideration of games in which information available to one player is hidden from the other. Such games are matching pennies and any of the variants and simplifications of poker. Although the machine will play seriously in such games, it does have a weakness that a knowing human player could exploit: As is well known, it is desirable to "bluff" in poker—bid high on a few low hands; otherwise one's bet would tell the opponent the approximate value of one's hand. In playing against the machine, one could (at an initial cost) "see" all high bets independent of the value of his own hand. On the average, this would drive down the $\overline{F}$ for "high bet with low hand," and the machine would reject the bluff—as it should *as long as* the opponent holds to his costly strategy. If, now, the opponent shifts to the normal optimum strategy, the $\overline{F}$ for "low bet with low hand" will be unaffected in general, but the $\overline{F}$ for "bluff" will *also* remain at their previously established values because the machine no longer chooses to try bluffing. The opponent has thus gained (at a relatively brief initial cost) a permanent advantage which he would not have gained without knowing how the machine works. The situation in matching pennies is even simpler: the opponent need only keep his own record of the $\overline{F}$ in the machine; whenever $\overline{F}$ (heads) $\neq \overline{F}$ (tails) he can predict the machine's next choice and win a round "unfairly."

The latter example shows that there are games in which the machine should always completely ignore information in its memory (so that its play will always be random and thus unpredictable). The poker example brings out the curious fact that a move may sometimes be dangerous only because the opponent is playing *poorly*. A similar but eventually less serious situation may exist for a time even in "rational" games. Here, should a very skillful opponent suddenly replace a poor player, the $\overline{F}$ of the machine's best moves will be lowered most rapidly because these are the ones it is using most often; here, however, the situation would eventually be corrected.

Thus there are games and situations in which the machine may be at a disadvantage if it relies too literally on its memory. In a few cases, it should ignore the information entirely. Its adaptability might therefore be improved by providing some feature allowing it to play "less carefully" upon occasion; the computing circuits could continue to compile information as precise as before, and the intact and extended information could later be used should the need arise.

The natural place to insert such a mechanism would be the section of the machine which compares $\overline{F}$ in selecting moves. If this comparator were temporarily made less accurate (for example, by using only the first few digits of the $\overline{F}$), the machine would play with somewhat less care, and, if the comparator were made essentially inoperative, it would play at random as though its memory had been entirely cleared.

How should the accuracy, C, say, of the comparator be controlled? It might at first appear that C should somehow be set in accord with the average recent income. However, any scheme which sets C in any predictable manner is prone to the following defect. In a rational game, the opponent could consistently "throw" a few games whenever C was set in its random-play position and thereby give the machine the false impression that it is playing better when it plays at random.

There are several methods of assuring that this situation cannot be stable,|| but the most direct of these would seem to be the following: Instead of setting the value of C by some formula, the machine could change C at random, with only the *frequencies* of the various positions determined. The basis for determining these frequencies would naturally be the average recent income—which could be computed separately for each setting C by the same method adopted above for the $\overline{F}$. There is considerable freedom in choosing a method for computing the weights from the incomes, but a vital requirement is that no value of C should ever be assigned zero weight (this avoids stability in the above situation). In accord with the intuitive notion that, in any situation, there is one "correct" value of C, we shall arbitrarily adopt the scheme incorporated in the summary below.

The comparator section, which selects the highest $\overline{F}$, is to have several settings C varying in accuracy from zero to that of the computer. At the beginning of each game, the value of C is set at random by a process wherein the frequencies of the various settings are determined as follows: Separate records of average recent income¶ I_C are kept for each setting of C. Those settings the incomes of which are lower than the maximum I_C are assigned a relative weight unity, and the setting with maximum I_C is** assigned a relative weight M (considerably greater than unity). At the beginning of "life" the machine selects settings of C at random among those not yet used until values have been established for all I_C.

The availability of this feature suggests two further mechanisms, which we have already anticipated. First, the value F_0 assigned to untested moves may conveniently be taken as the value of I_C for the "zero accuracy" position of C (which must start in this position if F_0 is to be initially defined). Second, computing $\overline{F}$ on the basis of constant N will now tend to encourage experimentation in early "life."

The design has now reached a stage of complexity where it is obviously very difficult to assess its "intelligence" in any purely theoretical manner. Evidently the comparator will be set in the proper position most of the time, but games will occasionally be lost while the machine, in effect, assures itself that other settings are not better. It cannot be stated how great this loss will be in the general run of game types; to some extent this depends upon the quantity M introduced above. One would expect that the resultant loss could be made a small price to pay for the great increase in adaptability over the earlier design. (The question naturally arises whether human tendency to error is an expression of this or a similar compromise.)

Even assuming that these quantitative questions can be answered in favor of the machine, there remain at least a few imperfections. How important they may be, it is not easy to tell, for one begins to wonder what kind of a showing a human being would make if forced to learn games by the totally blind procedure required of the machine. Be this as it may, there are certain types of games in which the machine definitely cannot find the best strategy. These are games where certain alternative moves should be mixed randomly but with *unequal* frequencies. Because the only provision for random play now involves equal weights for the alternative moves, such a strategy cannot be constructed. Since (as a few examples will readily show) the $\overline{F}$ do not even approximately indicate the correct move-weights in this type of game, extending the design to handle these cases properly would involve such enormous complications on an already barely practicable mechanism that the price would seem too great for a perhaps academic improvement.

Another imperfection may be brought out as follows. Suppose that, in an early game, the machine tries an actually excellent opening move but has the misfortune to follow this with an extremely disastrous mistake. The opening involved will then acquire a large, negative $\overline{F}$ and will therefore not be tried again for some time. It is unlikely but quite conceivable that the machine could then develop a creditable "game" excluding this one opening. By then the general level of the $\overline{F}$ will have risen to such an extent that that one excellent opening move is effectively excluded for the "life" of the machine. (The analogy to a neurosis is very tempting but hardly compelling.)

A final imperfection may be noted. There is no built-in provision whereby the machine can determine what it may expect to win if it learns to play well; it has no goal. This was intentionally avoided both because the game may not be symmetric or "fair," and also because the amount to be expected really varies with the skill of the opponent. The net result, however, is that the machine, having

|| Recall that the computer continually compiles "fully" accurate information whatever the setting of the comparator.

¶ In the same sense as the F—i.e., winnings per move.

** If this value of C is not unique, a further random selection with equal weights is made between such values, just as is done with the F.

discovered that certain moves are better than others, may become content with an actually low income rather than risk the experimentation needed to search out improvements. Again, analogies in human behavior are not difficult to find.

Although the methods of this inquiry have led to some clarification of the special problem considered and to a design apparently capable of exhibiting considerable "intelligence," they possess the inherent disadvantage that some very important possibilities may well have been overlooked. From a logical point of view, our conclusions stand upon very shaky foundations indeed. It seems safe to conclude, however, that a relatively simple over-all plan, requiring comparatively little computation, can suffice to give a machine somewhat more skill and intelligent adaptability than might at first be supposed. Perhaps an empirical test of a simple machine of this type in actual play against humans (some of whom might be forced to learn the game during play as does the machine) could lead to a better understanding of a few of the basic problems involved in any mechanistic interpretation of animate intelligence.

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SMO ON THE AIR

Station	Sponsor	Time
	Sunday	
KLBM, La Grande, Ore.	Baker-Union Department of Health (Research Report)	10:15 A.M.
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.
	(Irregular)	
KBER, Baker, Ore.	Baker-Union Department of Health (Research Report)	. . . .

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Cave Pearls in Carlsbad Caverns

DONALD M. BLACK

The author is now a park ranger at Grand Canyon, following a similar assignment at Carlsbad Caverns, New Mexico. It is evident that Mr. Black made good use of the intervals between tourist calls of "Oh, Ranger!" to observe some of the interesting minutiae of cavern development.

HESS[1] considered oolites as tiny, somewhat rounded concretions formed of concentric layers of minerals. When several of these are grouped together, they resemble the roe of fish —hence the name oolite, from the Greek, meaning "egg." DeFord and Waldschmidt[2] define oolite as a rock made up of an aggregate of "ooliths" having an average normal diameter of less than 1 mm. If the average diameter is greater than 1 mm, and less than 10 mm, the rock is "pisolite," and the individual spheroids are "pisoliths;" *piso* is also from the Greek, meaning "pea." Cloud, Barnes, and Bridge[3] propose the term "ooid" for the individual tiny spheroids in an oolitic rock. Dana's[4] definition states that oolite is a granular limestone consisting of minute concretions somewhat similar to the roe of fish. Twenhofel[5] uses the term to indicate accretions that have distinct concentric bands of radial crystalline growth; those that have a predominant radial growth he classifies as "spherulites."

I prefer the original use of the term oolite, meaning accretions having egglike shapes and surface textures, without reference to internal structure. Accretions described in this study have definite, well-defined layers of growth, and they would be classed by Twenhofel or Hess as oolites. Since Hess has established the term oolite in his study of the Carlsbad Caverns (New Mexico) accretions, I shall continue to use this term in reference to spherical, or nearly spherical, laminated accretions.

The term "cave pearl" was introduced by W. T. Lee in describing oolites he had collected in the Carlsbad Caverns in 1925. The term was later put into print by Hess[1] in his excellent article describing the cave pearls and other accretions collected by Dr. Lee.

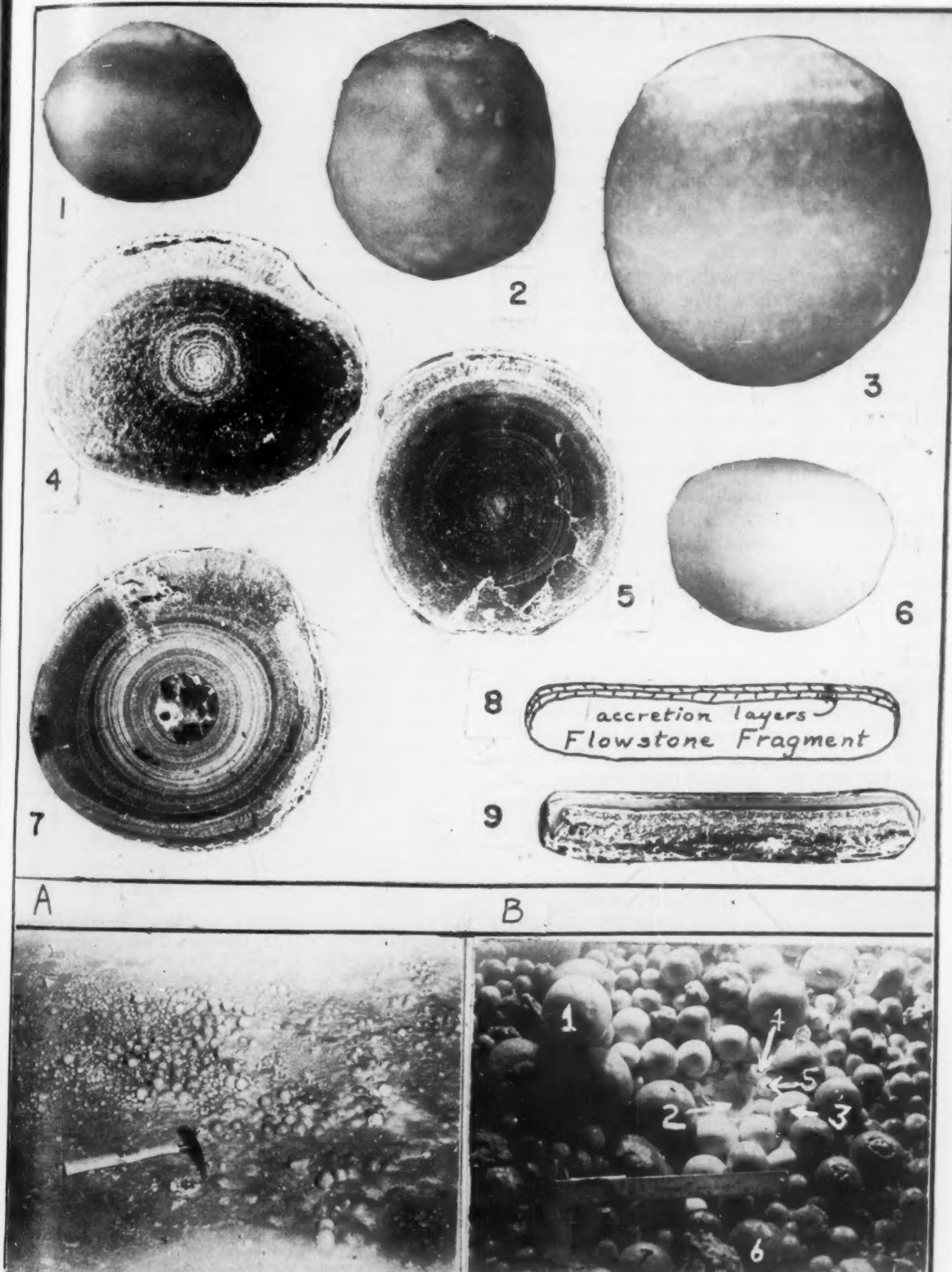
Most of the reports concerning cave pearls, oolites, and loose carbonate accretions found in caves have not specified whether the formations observed were in place, or whether they had been carried there by currents of water. The failure of observers to establish this fact has led many of them blindly to accept the universal theory that dripping water and water currents are responsible for rotating a nucleus while it grows into an oolite, or cave pearl. Twenhofel[6] once stated that he believed there were probably still unthought-of ways in which oolites might be formed. The purpose of this paper is to fulfill, in part, that prediction.

This study of the Carlsbad accretions has consisted of three steps. When an oolite or other accretion was found, the general area was studied; next, the exact spot on which the accretion sat was examined; then the cave pearl was carefully observed as to shape, size, and surface texture. Accretions were always found associated with seeps, stream beds, or water basins. The removal of accretions by early visitors to the Caverns has nearly depleted the original nests of them. Only one large nest (about four feet in diameter) and six small catchment basins (each less than a foot in diameter) were located; these were apparently natural and undisturbed.

The largest nest had formed on a gently sloping flowstone floor in the Devil's Den area (Fig. 1: *A*). It had a somewhat circular periphery, and a cross section with a slight upward convexity. A thin film of seep water and an occasional drop of water from the ceiling were its only sources of moisture. The dripping water would fall, strike an accretion (Fig. 1: *B*, 1), and form a fine spray. Accretions within the radius of this spray were definitely whiter and more translucent than other accretions not reached

FIG. 1: 1 and 4, Ovate accretion with accretion caps on top and base (× 1.7 and × 2.4, respectively). 2, Accretion with cap and sides tending toward tuberculation (× 1.5). 3 and 5, Accretion with cap; regular pattern of cracks on sides caused by dehydration when the accretion was removed from the Caverns (× 2.3 and × 1.8, respectively). 6, Lateral view of ovate accretion with eggshell texture; growth on top overhangs the base (× 1.3). 7, Cross section of prismatic accretion. Inner dark circle is cross section of a bell cord type of stalactite. Inner growth accumulations are more regular than the irregular outer layers of accretion (× 2.7). 8 and 9, Diagram and cross section of flat fragment showing nature of layers of growth (× 2.5).

A: General view of the large nest. Scale indicated by prospector's pick in foreground. *B*: Close-up view of a small section of the large nest. 1, A nearly perfect pearl with several small depressions in its surface. 2, Seat in which pearl "1" normally sits. 3, Depressions in the surface of a pearl. 4 and 5, Small depressions conforming to the curvature of pearl "1." 6 and 7, Coarse accretionary surfaces on tops of nonrotating accretions.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by it. The nest contained a very low percentage of tuberculated accretions; most of them were smooth or had the texture of an eggshell, or of very fine sandpaper. Shapes varied from spherical to irregular; some had "caps" of accretion (Fig. 2: 1–4) and depressions of suppression or abrasion (Fig. 2: 8, 9, and 11). The largest pearl in the nest was about 1½ inches in diameter (Fig. 1: *B*, 1). Hundreds of accretions were cemented to the floor. Some were on the periphery; others, within the nest, were arranged and deformed so as to form seats for other accretions (Fig. 1: *B*, 2).

Deeper water, basin walls, small diameters, and tuberculated accretions (Fig. 2: 16) characterized the smaller nests. They were formed on steep talus and flowstone slopes, and contained from three to six pearls each. A few of the pearls had migrated several feet down the slopes from the nests. The water in the nests covered the pearls to about mid-diameter, and the basins were definitely supplied by dripping water.

It is not always possible to determine whether accretions are "in place." When they sit on a flowstone floor, or are associated with gravels and sands of non-oolitic nature, it is probable that they have been washed there by water currents. In rainy seasons enough water often floods through crevices to transport accretions by traction. Accretions in place usually sit in a depression, or "seat" (Fig. 2: 8; Fig. 1: *B*, 2), which conforms to the curvature of the accretion. In a large, undisturbed nest, many of the topmost accretions lie in seats in the upper surfaces of lower forms that are in contact with the floor (Fig. 2: 8 and 9). Accretions having lateral contact with others often develop depressions in their surfaces conforming to the curvature of the adjacent ones (Fig. 1: *B*, 1). For simplicity, the shapes of accretions might be classed as "irregular," "prismatic," and "spherical."[7]

Irregular accretions have nuclei of fragments of bedrock or irregular fragments from deteriorating formations. Such accretions are common to most of the small drip pools throughout the Carlsbad Caverns.

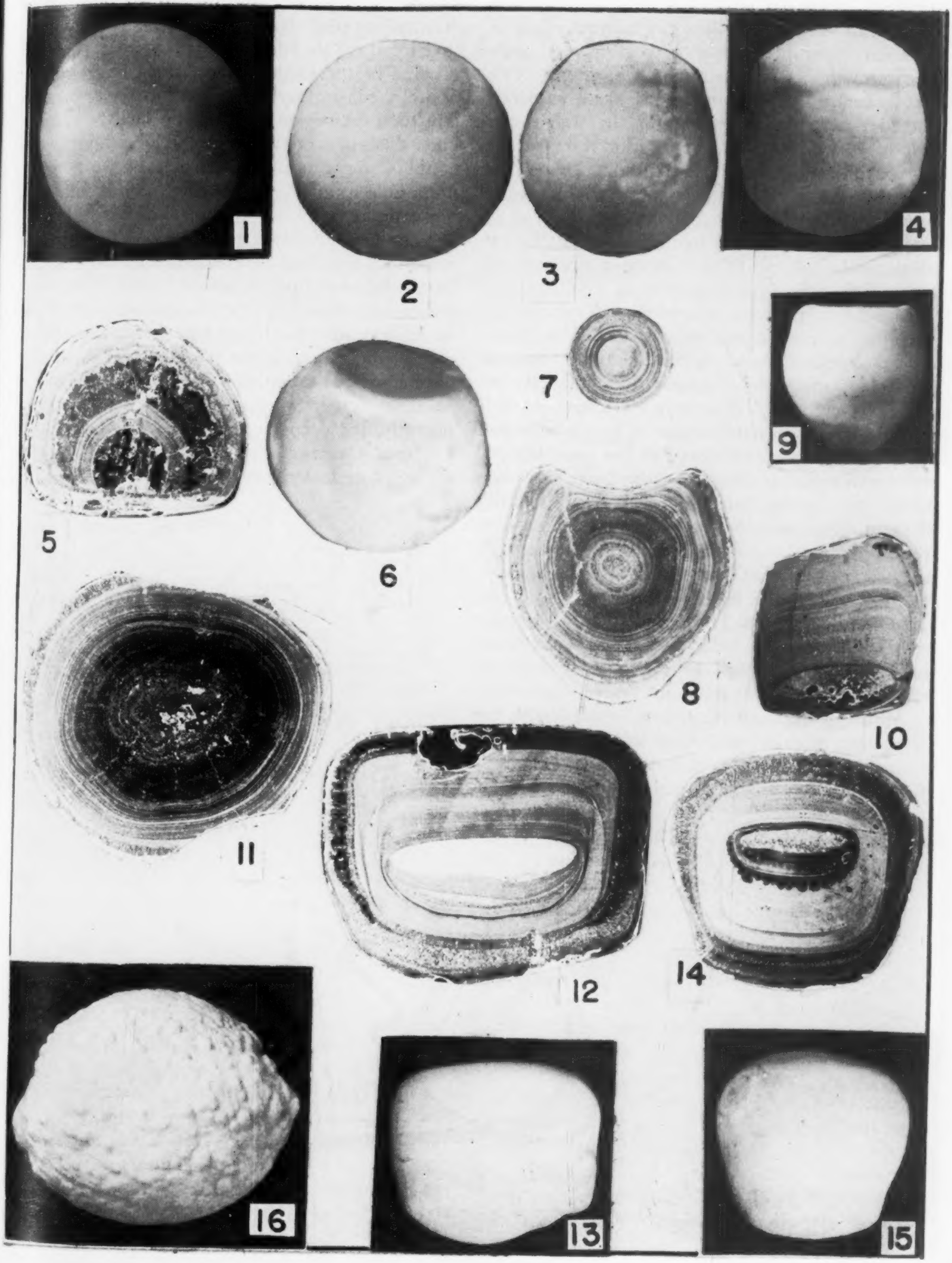
Prismatic accretions form around prismatic fragments of stalactites or around flat scales from other formations. Of all the accretions studied, these offer the most information concerning carbonate deposition. Increments of growth around the prismatic types are highly variable (Fig. 2: 5; Fig. 1: 7). The rate of growth is much faster on the top surface than elsewhere. On prismatic fragments, the layers of growth form as irregular, invaginated crescents with an upward convexity (Fig. 2: 5). Flat fragments accumulate rapidly on their upper surfaces, and very slowly on their sides and bases (Fig. 1: 8 and 9).

Spherical accretions have a tiny nucleus, usually consisting of a single grain of sand or silt. Their layers of growth are nearly concentric (Fig. 2: 7). Spheres become elongated or deformed by "caps" or areas of excessive growth.

Depressions are either primary or secondary. Primary ones develop when a surface of a growing accretion is suppressed by contact with another accretion (Fig. 2: 8 and 9). Secondary depressions are caused by abrasion; inner shells of accretion are cut and appear as concentric circles (Fig. 2: 11).

Hypothesis of Formation and Growth. Since the top surface of a nonrotating scale grows much faster than its sides or base, it would be safe to assume that mineral concentrates travel downward and at right angles to the water surface. If a completely submerged accretion accumulates minerals more rapidly on its top surface, or above mid-diameter, it becomes topheavy and overbalances to adjust its center of gravity. If an accretion is only partially submerged (as observed in the large nest), water migrates over its exposed surfaces and deposits minerals as long as evaporation removes moisture from its upper limits of migration. Some pearls probably originate around a submerged nucleus and eventually grow to a diameter equal to the depth of the water. Beyond this diameter, growth is achieved by evaporation; "adhesion" water climbs the exposed surfaces, evaporates, and deposits minerals as thin crescent-shaped laminations. These add weight to the top and eventually cause the accretions to rotate in adjustment to their newly acquired weights. The small accretion caps that form on the submerged bases are due to precipitation from the water (Fig. 2: 8).

FIG. 2: 1, Note very thin accretion cap on upper surface of oolite (× 1.5). 2, Cave pearl with very small accretion cap on top (× 1.5). 3, Ovoid accretion with well-developed cap on top (× 1.5). 4, Accretion with well-developed cap on top and bottom (× 2). 5, Cross section of elongated, or prismatic, accretion with stalactite core. The lower side of nucleus was broken and kept accretion from turning during its growth. (See Hess for other views of this type of accretion.) (× 3.1). 6, Accretion with depressions (× 2). 7, Cross section of cave pearl (× 5). 8 and 9, Cross section and lateral views of an active accretion; growth rings are suppressed on its upper surface. Note the small cap of growth on its base (× 2.3 and × 2, respectively). 10, Small flat fragment with excessive growth layers on upper surface (× 3). 11, Cave pearl with secondary depressions of abrasion (× 2.7). 12 and 13, Cross section and lateral views of a flat-cored accretion; note the number of times this accretion has been turned during growth (× 2.2 and × 1.5, respectively). 14 and 15, Cross section and lateral views of a flat-cored accretion (× 2 and × 1.7, respectively). 16, Tuberculated accretion (× 2).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Adhesion water is the lubricant for accretion rotation. Small pearls offer a large surface area per unit of weight; therefore the buoyant effect of adhesion water permits them to turn very easily when growth changes their centers of gravity. This fact accounts for the numerous small perfectly shaped pearls. As a pearl grows, its weight per unit of surface area increases until it reaches a climax and resists rotation. This diameter varies from 1 to 5 cm, depending upon available water, rate and height of zone of evaporation, type of seat, and character of adjacent accretions. Under ideal conditions, pearls will attain a diameter of about 3 cm before resisting rotation.

Inability to rotate is no criterion as to when an accretion will become attached or fixed in place; several dozens of them, in the larger nest, were so deformed as to prohibit rotation, yet they have remained unattached. Practically all accretions will become attached when the zone of evaporation and mineral deposition is lowered to the point of contact between the accretion and the floor. There are many areas in the Queen's Chamber and The Big Room that are slightly depressed, and coated with rough attached botryoidal accretions. Many of these areas are inactive nests of cave pearls comparable, in size and topography, to the large nest previously described. The pearls in these inactive nests, however, have become attached to the floor; subsequent growth has obliterated their spherical shapes with irregular coats of carbonate minerals.

Although dripping water is associated with the large nest of cave pearls, and might be the force turning the pearl it strikes, it cannot be responsible for the rotation of pearls outside the zone of dripping. Dripping water agitates the surface tension of pools and permits the downward diffusion of mineral concentrates that would normally form cave ice along the shoreline.

Some of the Carlsbad accretions have smooth, or polished, surfaces. This texture is apparently due to a deposition factor. The lack of exposed inner shells of accretions, and the lubricating action of the water around fine particles make it nearly impossible to attribute their high polish to a buffing action.

The theory of origin and location of oolitic cave accretions described here agrees with those of Casteret,[8] Mackin and Coombs,[9] and Twenhofel, Keller, and Hess only in that oolitic cave accretions do have definite nuclei, and that they grow by accretion of definite layers of minerals. Twenhofel[6] once expressed an opinion that each occurrence of oolitic structures was an individual problem. Apparently, the occurrence of such structures in the Carlsbad Caverns does present factors indicating a heretofore unaccepted process of rotation.

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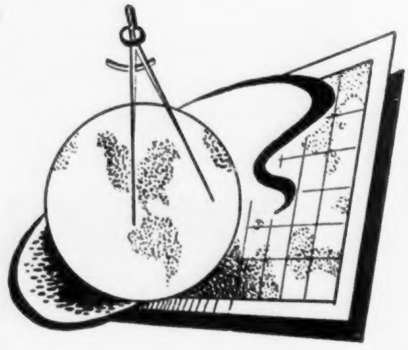

Books, Battles, and Biology

L. P. COONEN

Dr. Coonen presents here a further chapter in his projected book on the history of biology. "The Prehistoric Roots of Biology," which appeared last September in THE SCIENTIFIC MONTHLY (**73**, *154* [*1951*]), *covered the history of man as we have been able to glean it from the scattered bits of biological evidence left to us. In his present article he deals mainly with the story of Babylonia. Dr. Coonen is head of the Department of Biology at the University of Detroit.*

NEOLITHIC man's practical and aesthetic biology was somehow inherited by historical man. Students of ancient history do not agree on the time and the place of the legacy's consummation.[1] Nor do they agree on how the bequest was transferred. Perhaps it was done by the simple expedient of demonstration, followed much later by elaborated systems of communication.

Early historical man certainly developed speech, and he invented writing. The latter was no doubt a slow, evolutionary process. It may have started with pictographs; in some instances it proceeded to hieroglyphic characters or idiography; thence on occasion, via syllabism, to the alphabet. Progress may have been accruing in various loci, and not all running the gamut to the alphabet. Even the ancient Mexicans, before the advent of the Spaniards, employed picture-writing to spell out the names of persons and places.[2] Writing seems to have been developed independently, and to have risen to various heights, in Phoenicia, China, and Egypt.[3]

If counting noses of historians solves the problem, then we should at least state that somewhere, sometime, in the lands between the Tigris and the Euphrates thoughts first became "fossilized" on bricks, clay tablets, or stone columns. There a system of symbols was evolved to express a flow of mental activities.

Writing, the Staff of Civilization

Experience then became bankable coin. Ideas, observations, procedures, and conclusions could be transmitted because writing formed a network of communication between thinkers. And, perhaps more important, writing held the mental focus of a thinker indefinitely, to be returned to again and again. The wise and the ingenious were not isolated and intellectually lonely, and valuable precedent could be tapped from "storage."

The system of collecting and arranging writings into libraries was to develop later, but already in Mesopotamia's Nippur, probably 5000–6000 B. C., clay tablets were being collected in special rooms. Here in Mesopotamia we find scraps of the earliest biology recorded as civilizations were emerging from the fog of prehistory. Writing, the wonderful new medium, was growing and maturing; with communications being established and improved, progress was assured and inevitable; knowledge could be cumulative. Diffusion and exchange of ideas had been started. Velocity and expansion were the new attributes of written thoughts. And the potential was to be this: all men who read, and all men who will be able to read in the future, for all the unspun aeons of time, can join forces. The original idea can be and shall be recaptured and embellished. New overtones will be, or can be, added by yet-to-be-born thinkers and observers. Always, that first mental explosion flies outward and onward because it is written. Ideas are now born to immortality; in human skulls they will echo till the end of time.

And, thanks to writing, biology is on its way.

War, the Deterrent to Ancient Cultures

Civilization was to take an unpredicted turn, however. The history of "civilized" peoples became a story of war and destruction. Brain boxes became steppingstones for little men who would go far. Too often pride and avarice became the prods of leaders. Aggression and suppression were destined to be sweeping and inviolate erasers of the works of the pacific. Sinister fires of human ambition flared and burned, stoked with buildings, books, and the bodies of men. Real progress was spasmodic.

As always, peoples rise and fall with the strong men who lead, and, with few exceptions, dictators have destroyed themselves. This is nicely stated by the anonymous "B. R." in a marginal comment in his sixteenth-century translation of Herodotus.[4] With the cryptic eloquence of doom his quill traces out the warning, "Whom destinies wil have die he shal be the busie worker of his owne peril."

And so civilizations have flourished and faded. Sometimes they have taken with them to final oblivion their writings or the keys to our understanding them. Thus the essence of their expensive experiences was lost.

The cradle of Western civilization cannot be pointed to with certainty. More and more evidence seems to floodlight the Mesopotamian plains, between the Tigris and the Euphrates, as the site of the beginning of the story of Western culture. There the suckling civilization nestled obscurely, in the rough, crescent-shaped cradle which enfolds the eastern end of the Mediterranean.

With time's perspective we smile at rulers and subjects alike, arrogant in their security, who built monuments and walled cities, and then disdained all unfortunates born extramurally. In that human and egocentric way, theirs was, they were convinced, the charge of the gods to carry on forever. Some of those inbred human clones survived all difficulties for centuries. But fatness and fortuity are friends. Eventually lean foreigners rushed in to wrench away sickles and bend swords, a surprise to everyone. Perhaps they were among the first groups to learn that political security is never to be passively enjoyed. But history was to be full of surprises. Structures of security conceived and executed with meticulous care and elaborate plans suddenly crumbled. The likelihood of annihilation could not have been accepted in downtown Nineveh by local merchants on any given spring day in 650 B. C., or in Nippur 3000 years before.

To and fro, the conquerers and the conquered surged across the dusty map of a little world no larger than the state of Montana. At times the triumphant at arms was only superficially a victor. He was the loser in a more significant battle—that of germ plasm expression. His peculiar characteristics were merged with those of the more numerous vanquished peoples and thus became lost, or diluted beyond recognition. Absorption, diffusion, and dissolution have been the fate of many winning armies.

The roster of civilization, each proud and defiant, is dim and confused after thirty-five centuries. Perhaps some of the political entities which endured longer than the United States are not even on the call list, for no other reason than they have been forgotten. The mightiest peoples in the world —for one or a few centuries, or even a millennium —have left only ashes and crumbling stones beneath the impatient sands of an unknown or forsaken land. Even the stream of immortal germ plasm seems to have dried up or altered. Sumerians, Babylonians, Chaldeans, Scythians, ancient Persians, Medes, Libyans, Assyrians, Urartus, and the more recent Greeks and Romans: what has become of them? Were they all fathers without sons? Not one of the proud genealogies, exalted by those ancient powers, is traceable to our times.

From the parade of uneasy and pulsating kingdoms some historical fragments have come down to us. Among them are precious bits of laborious writing suggesting an outline of early biology. It is evident that the hunting and fishing of Neolithic times were never completely abandoned. Iron and bronze weapons were still cherished and prized. But they became complementary to the hoe and the plow. The common man became more and more dependent upon cultivated plants. He leaned more heavily for his fats and proteins upon his domestic animals, although he continued to hunt the wild ones. Wanton, rapacious "zoology" was the hobby of only the royal huntsman. As recently as 100 B. C., King Tiglath-pileser I of Assyria boasted, "I killed 120 lions . . . on my own feet, and 800 lions I killed from my chariot."[5] The same ruler, in a more kindly nod to zoology, made a gift of a crocodile[5]—probably to a zoo.

We are not surprised to find bold evidences of interest in animals in the early chapters of history. It is something of a pleasant surprise, however, to read further and learn that this same robust ruler introduced cedars and other trees into his kingdom.[6]

Early Written Biology

Without too much regard for the exact sequence of civilizations, or even for the order of events, let us discuss biological records and happenings that occurred in Mesopotamia. The geographical entity Mesopotamia, as here loosely employed, includes besides the interriver triangle of lands, those immediately adjacent to the outer banks of these streams. Its present status is a sad footnote, not only to the ephemeral nature of cultural and political entities, but even to the geography that allows them to exist. Today's swamps and deserts make it difficult to realize that this was a land once chosen and fought for when good lands were plentiful and choices were wide (Fig. 1).

Lower Mesopotamia was occupied early by the ancient Sumerians, a non-Semitic people with a civilization already settled as early as 4000 B. C. A civilization with limited power in the hands of the ruler existed here; this was quite different from the political and domestic environment in Egypt. About 2000 B. C. we find some evidence of scientific observation in this relatively favorable political climate. Besides astronomical tracts, documents of that time have been found listing the names of hundreds of

FIG. 1. Mesopotamia, the cradle of civilizations, where several of the earliest cultures sprouted, bloomed, and died. Here were perhaps the world's first villages, cities, wars, and written records, including biological data. Some sites contain stratified debris of several settlements, each one built upon the ruins of its predecessor. One such mound, Tepe Gawra, near Nineveh, has been found to contain evidence of twenty-six settlements, spanning more than 3000 years.

kinds of birds, fishes, and domestic plants and animals.[7]

The new skill of writing was quite generally employed at first to record ownership of property in this land, where ownership was a unique right of the time. The lists served as a pictographic key, or code, with some of the functions of a dictionary. Because the groupings were natural they suggest some fundamental thoughts on taxonomy.[7]

One very early record, an engraved seal, found in Elam, near Ur, shows horizontal rows of horse heads in caricature (Fig. 2). Three types of profiles—straight, concave, and convex—are represented; and pendant and upright manes—or none—are shown. These rows of pictographs, with contrasting variations in stark relief, suggest a pedigree record of horses, probably of the fourth millennium B. C.[8] At least there is the possibility that already in the dim dawn of history a practical "geneticist" was compiling phenotype records.

Many contemporary botanical terms and plant names were first spelled out by Sumerian or As-

syrian scribes. Although this may have been done as long as forty centuries ago, we retain many of them in our present vocabularies. Crocus, saffron, chicory, cassia, hyssop, myrrh, cumin,[7] poppy, and sesame,[8] are examples.

Still another Mesopotamian record, definitely Sumerian,[9] was laboriously carved on a column of limestone by some unknown, chiseling "stenographer." It has bacteriological and pathological significance, at least by innuendo, and is most often referred to as the "Stele of Vultures." The name has stuck because the rock column honors those avian scavengers as important factors in battlefield sanitation. The Sumerians considered war sanitation so important that three points were committed to this permanent record: (1) vultures were effective in removing carnal wastes from the battlefield; (2) the common grave was a helpful expedient in curbing diseases during military campaigns; (3) the practice of gathering up the fallen soldiers and dressing their wounds, a precursor of the field hospital, was set up and recommended. Thus it seems that the health problem was recognized and met with an eye to controlling infection and disease sources, all the more remarkable if we pause to reflect on the date, 2920 B. C.

Babylonia, on the eastern bank of the Euphrates, was a breeding ground of restless and ingenious peoples of Semitic origin. They met with recurring strife between their various factions. Wave against wave of bloody groups killed or absorbed each other, subsided, and settled. Then they traded peacefully for decades, rose again at each other's throats, only to pass again into a period of comparative quiet. In 2750 B. C. Sargon rose to the leadership of these Semitic tribes, united them, and showed them that they could beat down forever their common rivals, the Sumerians. His new realm, the Sumerian Akkadian empire, dominated the world incontestably for over two centuries. Motivated by wisdom or hard-headed expediency, the Semites took over the language and the more or less facile cuneiform writing of the conquered. Sumerian was to remain the language of power and learning for centuries.

Even muscles hardened by war cannot remain flexed forever, however, and the ruling mind imperceptibly softens as it assumes its own invincibility. Deterioration, physical and psychological, invited hard-knuckled neighbors to move in—not one, but two. From the east came the Elamites, of uncertain origin; from the west, the Amorites. Caught in the vise, and gnawed at from both flanks, the Akkadian empire was debilitated. It slowly succumbed to anemia.

The Amorites moved in, chose the small town of Babylon as headquarters and, after a hundred years of sporadic fighting, took over all of Mesopotamia. The time was 2100 B. C.; the first king, the great Hammurabi; his new domain, the First Babylonian Empire.

We are permitted a searching look at this cultural arena, for here the groundwork, from which our civilization has risen, was conceived and initiated. Perhaps some of the fundamentals of Oriental civilizations were launched here, too. The core of our mathematics and astronomy is traceable to Babylonia, and here our decimal system was sired. Here units of mensuration were invented: the year was divided into twelve months, the circle into 360°, the week into seven days, the hour into 60 minutes, the minute into 60 seconds. The fundamentals of music, architecture, and carpetmaking either originated or were wisely developed here.

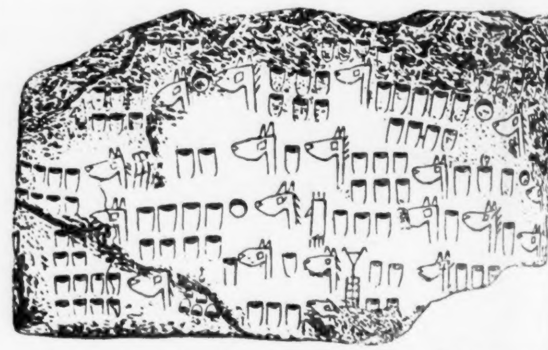

FIG. 2. Horse-breeding record of about 6000 years ago engraved on a seal, from an excavation east of Ur, in Elam. Note three varieties of profiles: straight, concave, and convex; and the upright and pendant manes—or no manes at all. (From W. Amschler, after de Mecquenem and von Scheil.)

And under the wonderful Babylonians writing seems to have been further streamlined.

The Assyrians, another Semitic group, looked down from the north. They also were destined to have a try at running Mesopotamia. After having quarreled with the Hittites and other next-door peoples for many years, they became stronger and felt bold enough to take on the champion of champions, Babylonia. When finally they introduced the horse and chariot as implements of war, theirs was the glory of easy victory. From Nineveh they moved their capital to fabulous and subdued Babylon. Thus Tiglath-pileser I, an Assyrian, became the new master of Babylonia, and literally "King of the World."

In 606 B. C. there was to be yet another Babylonian empire. This grab was engineered by the Chaldeans and was publicized as the Second Babylonian Empire. Then came Cyrus the Persian, and later

yet, Alexander the Great—but not for us to deal with here.

Biology, Astrology, and Medicine

We must think now in terms of the "great land between the rivers" and what it as an entity gave to biology while snitchers and snatchers with cacophonous names were raping the cities and maiming the faceless citizenry. To imply that the biological sciences received a forceful impetus in any one of these early civilizations would be wishful thinking. Although there was some biological observation such as that which inspired lists of animals, insects, and plants and of the different kinds of lice, bugs, and flies to be recorded in cuneiform inscriptions, such lists were not taxonomy, nor did they become progressively more scientific. One Assyrian herbal of the seventh century B. C., according to Reed,[6] showed a fair knowledge of medicinal and nonmedicinal plants, but the classification was sometimes chaotic—no better than an alphabetical arrangement. For instance, the composites, comprising the highest family of flowering plants, were scattered throughout the list.

Seelig[10] says Egypt was the source of medical biology, corrupted and confused here in Mesopotamia by superstition and theurgy. But who can prove this?

Astrology, today relegated to the idle and the credulous, was a murky mist over the minds of thinking men. Epidemics, death, war, famines, and private good or bad fortune, supposedly could be predicted by interpreting the celestial signs. Heavenly bodies in their various positions, influenced the lives and the affairs of men. We inherit the words "disaster" (forsaken by the stars) and "lunatic" from this era. Mingled with these sidereal variables were those of local weather and certain abnormalities of birth. Here are a few:

> If Mercury makes its appearance on the fifteenth day of the month, there will be corpses in the land.

And, again,

> . . . if the constellation of cancer is obscured, a destructive demon will take possession of the land, and there will be corpses.
>
> If there should be thunder during the month of Tisri, a spirit of enmity will prevail in the land; and if it should rain during that month, both men and cattle will fall ill.
>
> If a woman gives birth to a child whose face resembles the beak of a bird, there will surely be peace in the land.[11]

To further complicate matters, mathematics became a bedfellow of astrology, making the depth and the ramifications of explanations and portents infinite, and giving mathematics a bad name. The evil reputation persisted for centuries; in the Theodosian code of 1500 years ago, mathematicians were given the death sentence. Even the Frenchman Abelard, 500 years later, called the science of mathematics a nefarious study.

Despite evidence to the contrary, Herodotus,[4] who visited Babylon in 300 B. C., reported that the Babylonians had no physicians. His account may cast some light on that country's medical practices in his day, a comparatively modern time. He states that the diseased and indisposed were placed in the public square so that the passers-by could compare notes with them on personal medical histories. Or perhaps they could describe the symptoms and successful treatment of a similar sickness in a relative or a friend. This open-air clearinghouse of moribundity evoked the use of worthless and even harmful elixirs. But, most important, the laws of chance and the innate common sense of man brought out some valuable therapeutic information. Here was supersocialized medicine on a primeval footing.

Perhaps that was a temporary corruption in Babylonia. We need always to be careful not to stamp the label of an ephemeral phase of history upon a thousand-year-old civilization or name merely because it held sway for a few decades. The "good old days," before Herodotus' time, had their surgeons and prescriptionists. A proud and doughty lot, they sometimes teased fate even more provocatively than did the patient who presented himself at their doors. The bumbling or unfortunate physician was dearly penalized for his errors. And, an additional insult, there was price control: fees were fixed by royal edict. All this was set in writing in 2250 B. C.[12] when Hammurabi, that first king of the first dynasty of Babylonia, promulgated the first code of laws. This was a thousand years before the great lawgiver Moses. Hammurabi had executed and erected an eight-foot, black diorite column (Fig. 3) on which was recorded a complete set of regulations: laws of property, marriage, divorce, wage scales, and dicta on medical practice, including those fees. Besides fragments of several copies of this stele, a large part of the original was found in 1901–2. About 1150 lines[13] of the original 4000 remain. Gaps have been filled in from fragments of copies found at Nippur and Nineveh.

Clause 215 designates fees:

> If a doctor has treated a gentleman . . . cured the man . . . or has cured the eye of the gentleman he shall take ten shekels of silver.

And note in Clause 218 the severe and final penalty for malpractice:

> If a doctor has treated a gentleman for a severe wound with a lancet of bronze and has caused the gentleman to die, or has opened an abscess of the eye . . . and has caused the loss of the gentleman's eye, one shall cut off his hands.

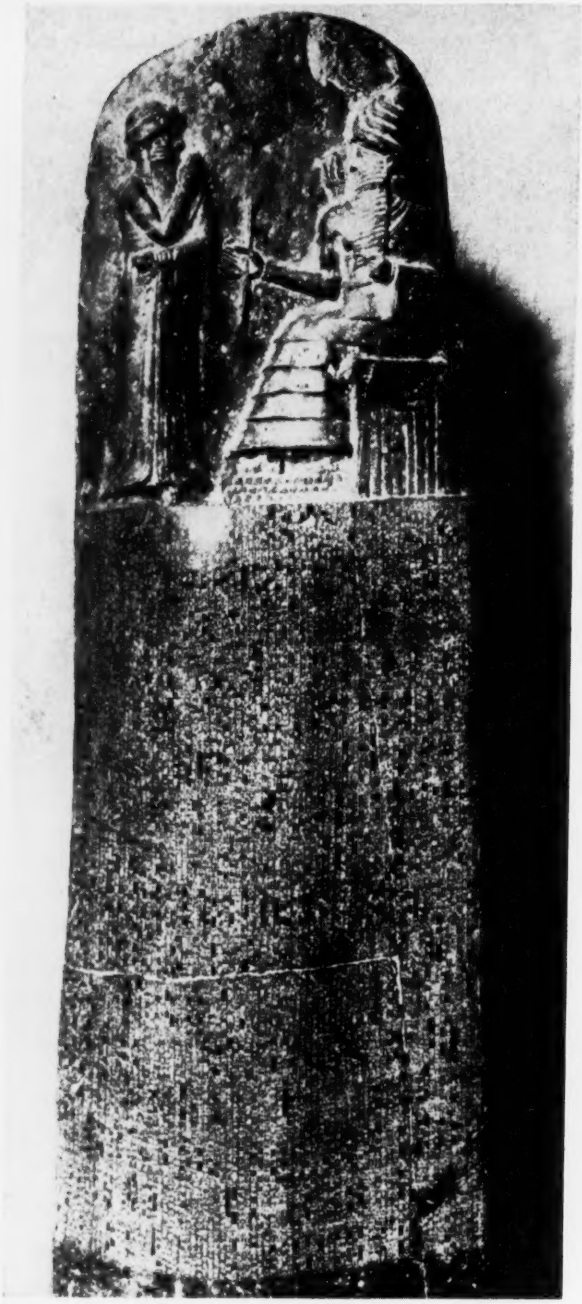

FIG. 3. The Hammurabi Code of laws inscribed upon an eight-foot black diorite column. These were Babylonia's laws about 4000 years ago. Copies of this stele were displayed in Babylon and other cities of the realm. Citizens could settle many of their legal difficulties by personally consulting these regulatory dicta. Specific fees for each type of medical treatment were stipulated, as well as the fines and penalties for medical malpractice. Atop the column King Hammurabi is depicted as receiving the laws from the seated sun-god. (From Harper.)

Reading the heavens, appeasing the gods, and dispossessing the demons probably pointed the way to another form of portentous reading. Hepatoscopy, the visual interpretation of the liver's topography, became a beacon tower for preferential living in all strata of Babylonian society. From the macrocosm to the microcosm was not a complete shift for portentious signs; it was a supplementary one. The livers of sacrificial animals were scrutinized for signs of good fortune or impending disaster. The fate of a nation seems to have hinged upon the subtle anatomical variations in a sheep's liver. Jastrow[14] has done extensive research on this organ's place in the history of medicine. Clay models of the organ were made and studied (Fig. 4) perhaps as early as 2000 B. C. No doubt they were among the earliest of anatomical models. If a Babylonian "expert," for example, examined a sheep's liver and found the gall bladder swollen on the right side, this meant that the king's army could look for greater strength. If the left side was enlarged, military disaster was in the offing. Gallstones were good or bad omens, involving other variables in their interpretation.

According to Osler,[13] the Bible makes at least one reference to hepatoscopy (Ezek. 21: 21): "For the king of Babylon stood in the highway, at the head of two ways, seeking divination . . . he inquired of the idols, and consulted entrails."

The liver seems to have occupied a place of concern and respect in the hierarchy of organs, much as the heart did later, and does now. Mesopotamian and, subsequently, Hebrew, Greek, Latin, and even modern bards used the word in speech and song to designate the seat of love, fortitude, and endurance.

Gods and demons figured largely in the cause and cure of disease in Assyria. In a letter from a medic, Arad-nana, to King Esarhaddon[15] an interfering deity is cited:

> It is very well indeed with this unfortunate man whose eyes were diseased. I took off the dressing . . . there was pus upon it the size of the tip of your little finger. Whoever of your gods has put his hand to this case has himself surely given his orders.

And in another letter between the same parties, the gods of healing are given their due:

> . . . with the oil of fish he anointed them. The king, my lord, is a worshipper of the gods. It is Ashur, Bel and Nabu who have helped you.

And another, from one Mardakshakinsham to the same monarch, acknowledges the role of the gods:

> Now I do not understand why he had this attack. Our report itself does not harmonize the one part with the other. The gods have done it.

Dentistry usually meets with elision in the meager historical sketches left to us. Here are two ref-

erences, however, which suggest some cognizance of oral pathology:

> Concerning the recovery of the tooth of which the king has written, I am improving very much the condition of the tooth. . . .
>
> The burning of his head, his hands and his feet, wherewith he burns is because of his teeth. His teeth should be withdrawn. . . .

The Assyro-Babylonian period is further characterized by some public hygiene and sanitation. The transmission of leprosy was recognized and lepers were expelled from their homes and communities. Huge drains were constructed to remove urban sewage.[9] Measures were taken to protect the health of royal and wealthy figures: a stone privy has been excavated from the palace of the fairly recent Sargon II (1300 B. C.).

Withal, the Assyro-Babylonian cultures were not conscious of biology as a sector of the science front and, aside from the lists of plants and animals referred to, their general interest in, and promotion of, the life sciences hinged upon personal or community needs. This was human biology; much of it was medicine. Practicality was still the hallmark. Even such fundamental discoveries as the sexuality of date palms[16] were engendered solely by practical aims of the grower. Results were appreciated; causes were not understood. The sex principle was incorrectly applied to other plants.[6]

A single bit of evidence, for whatever it is worth, indicates that some of these peoples speculated on human evoluton. A Sumerian tablet discloses this bold and startling phrase: ". . . when men walked on all fours and were beasts."[17]

In retrospect, then, the pre-Christian inhabitants of Mesopotamia made their greatest contributions by inventing a system or systems of writing, without which no science could have been nurtured or even perpetuated. They made some commendable approaches to battlefield and urban sanitation. And we note here and there a desultory glance at nature —an interest in seeing and naming and listing living things. As a backdrop, of course, were the plant cultivation and animal husbandry inherited from the Stone Age. To this mass of practical, workable knowledge new ideas were added. Besides the pollination procedure practiced in the date palm, a partial consciousness of genetics in domestic organisms is suggested.

FIG. 4. Clay model of sheep's liver with divination text for the Babylonian physician superimposed upon it. This is one of the oldest known biological models. Since the liver was considered the seat of life, it, of all the organs, was attuned to the will and designs of the gods. Hence, "reading" the liver was believed to disclose the secrets of the divine mind. Probably 2000 B. C. (From M. Jastrow, courtesy of College of Physicians of Philadelphia.)

Perhaps none of this was high-class biology, but it *was* deliberate recording of biological data.

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On Percentage Moments

Interpreting the Moments as Per Cents of their Maximums

STUART CARTER DODD

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FOR laymen interpreting the statistical moments, it is sometimes useful to express a moment as a percentage of its maximum. As per cents, laymen can understand and compare two distributions that are in different units as to relative frequency, or dispersion, or skewness, or kurtosis. Percentage indices in terms of the zero moment are in common use, but percentage indices for the higher moments are little known. All the moments may become more used if better understood, as in marshaling the percentage formulas and their geometric interpretation in Figure 1, together with a few comments on their uses and limitation.*

In order to express each moment as a per cent of its maximum in a given distribution, at least two properties of the distribution are taken as given. For these two properties, the population or area under the distribution curve, and the range from lowest to highest value of X, are chosen in this paper in general as given. Then the maximum value for each moment (except the first) becomes definite and can serve as a standard, or denominator, of the percentage moment.

The zeroth moment, defined by $\sum_1^N X^0/N = 1$, is a constant, of course. Relative frequencies are expressed in terms of it, as each such frequency is a proportion of the unit population. Thus the table of normal probabilities gives the familiar readings which are frequencies in terms of the zeroth moment. The percentage frequency, or simple per cent, is readily understood by laymen. It is not new and is noted here only for completeness in studying all the moments in percentage units.

$_{\%}N = 100f/N =$ percentage frequency, a per cent of the zeroth moment (where the presubscript denotes the units in which the base letter is expressed). (1)

The first moment about zero as origin ($\sum_1^N X^1/N = M =$ the mean) has no natural maximum in general, short of infinity. This makes expressing it in percentage terms unsatisfactory, in general. The mean may be expressed, however, in standard deviation units to increase comparability. This is the reciprocal of the coefficient of variation. Then the minimum value of the mean, in the case of symmetric distributions, is .5. This is reached in the symmetric dichotomous distribution where X is 0 or 1, so that the mean is .5, the sigma is .5, and the percentage mean (Eq. [2]) is zero. (For the non-symmetric case the raw mean can be less than .5 and even less than σ, yielding a percentage mean that is negative.) To convert to a percentage scale, take the reciprocal and subtract it from unity and multiply by 100. This "percentage mean" is a sort of complement of the coefficient of variation. It is minimal or zero in the symmetric dichotomy and approaches 100 per cent as the raw mean approaches infinity. For further interpretative examples, the "percentage mean" of a normal distribution whose zero is at the $-3\ \sigma$ truncation point is 57%, whereas it is 84 per cent when similarly computed for the normal curve in the world's current population (since $N = .23 \times 10^{10}$ and $M = 6.25\ \sigma$ when the zero point is at the lower extreme below the frequency of one person).

$_{\%}M = 100 - 100\ \sigma/M =$ the "percentage mean." (2)

The usefulness of this percentage mean seems limited, since it has the disadvantages (a) that the reciprocal has a curvilinear relation to the raw

* For some systematic uses of the five percentage moments see the author's "Historic Ideals Operationally Defined" (*Public Opinion Quart.*, **15**, (3), [1951]; and *Systematic Social Science.* Seattle, Wash.: Univ. Bookstore, Chap. 15 [1947]).

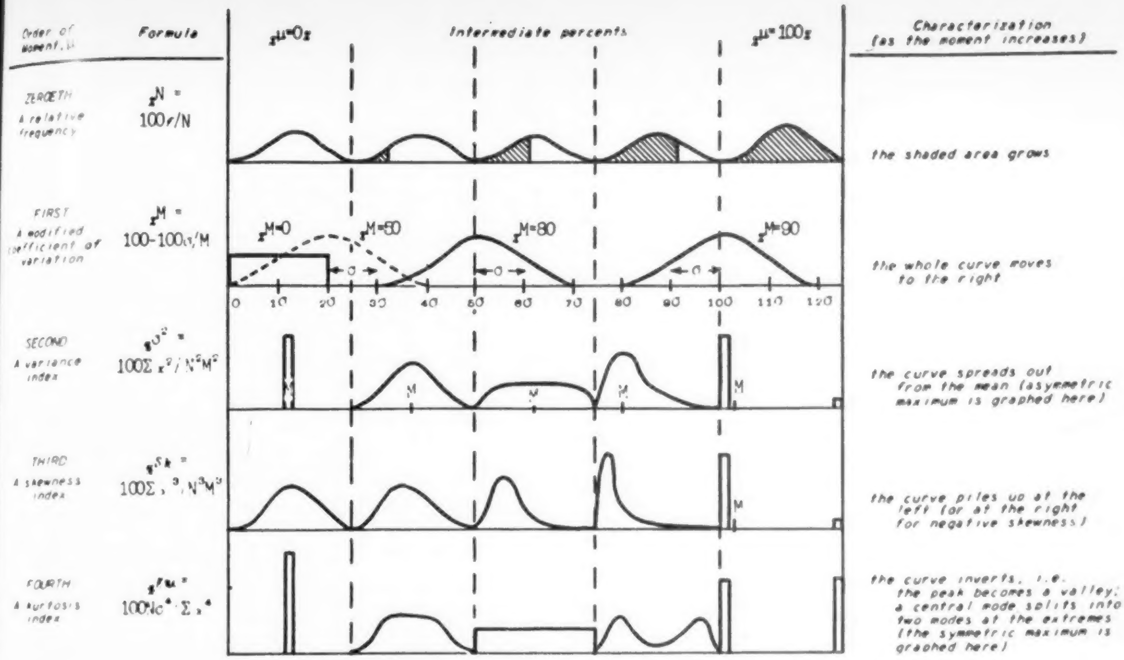

FIG. 1. The percentage moments, $_{\%}\mu$, diagrammed.

units, and (*b*) that it becomes negative if the mean is less than sigma. Like the coefficient of variation, the more arbitrary the zero point is the more slippery the percentage mean becomes. The concept of the arithmetic mean is commonly enough understood by laymen, so that this per cent form of it can be dispensed with. The same is not true, however, of the higher order moments.

The second moment about the mean ($\sum_1^N x^2/N = \sigma^2 =$ the variance) can be usefully expressed as a per cent of monopoly which is its maximum. In monopoly, the variable must be transferable such that one unit of frequency can have all the values of the variable (ΣX) while the other $N-1$ frequency units have none of it. The monopolist's deviation from the mean is $\Sigma X - \Sigma X/N$, and the $N-1$ other deviations are each $0 - \Sigma X/N$, or simply the mean. The standard deviation of the monopolistic distribution is then $(\Sigma X/N)\sqrt{N-1}$. Wherever N is large so that $N-1$ is negligibly different from N, the monopoly sigma is $\frac{\Sigma X}{N}\sqrt{N}$, or the mean times the square root of N. Expressing any observed sigma, then, as a per cent of its maximum gives the following equivalences:

$$_{\%}\sigma = 100\sigma/\sigma_{\max} = 100\frac{\sqrt{\Sigma x^2}}{\Sigma X}\bigg/\frac{\sqrt{N}}{\sqrt{N-1}}\ .$$

If N is large so that $N-1$ is negligibly different from N this becomes:

$$_{\%}\sigma = 100\frac{\sqrt{\Sigma x^2}}{\Sigma X} = \frac{100\sigma}{M\sqrt{N}} = \text{the "percentage sigma" (for } N \text{ large).} \quad (3)$$

$$100\sigma^2/\sigma^2 \text{ monopoly} = 100\ \Sigma x^2/M^2N^2 = {}_{\%}\sigma^2 = \text{"percentage variance."} \quad (4)$$

It should be noted that the maximum dispersion is also the maximum skewness, since both are reached only in a monopoly.

An example of one use of this percentage sigma† is to express the current dispersion of incomes in some population on a percentage scale between the limits of equal incomes for all and a monopoly. These are the limiting theoretic extremes of a communist economy and an unlimited competitive capitalist economy. Where on this scale are we, and whither are we moving? A rough indication by this index is that in 1947 the United States standard deviation of incomes was 2.5 per cent of a monopolistic one against 2.2 per cent in 1948—a small but possibly‡ significant shift in the equalizing direction.

Since no laymen and few statisticians can interpret the amount of skewing of a distribution curve merely from a knowledge of a number of some sort of units called the third moment, a percentage measure will make an amount of skewness more understandable. It is the per cent of the transition from any symmetric distribution curve to an

† For the percentage sigma, see S. C. Dodd. *Dimensions of Society*. New York: Macmillan, 297 (1942).

‡ The formula for the standard error of the difference of the two percentage sigmas needs to be worked out. Since the per cent variance is a ratio of two variances, Snedecor's F might be used except that it assumes normality of the variates. This does not hold for the denominator, which is the variance of a monopoly representing maximal skewness.

L-shaped curve (or a reversed-L in the case of negative skewness). Degrees of skewness of curves with differing shapes and units thus become roughly comparable in terms of percentage skewness.

The third moment about the mean ($\sum_1^N x^3/N =$ Sk = the skewness) can become a percentage of its maximum by expressing it in units of the absolute third moment (i.e., calculated from deviations, disregarding their algebraic sign). The absolute third moment about the mean can never be zero and is the upper limit of the algebraic moment. For at one limit, called symmetry or no-skewness, the algebraic moment is zero and the "percentage skewness" would be zero. At the other limit, which is the monopoly again, the monopolist's deviation is of one sign and all the other deviations are of the order of $1/N$ of it and become increasingly negligible as N gets large. For very large N the algebraic third moment almost catches up to the absolute third moment, but can never exceed it. Hence the algebraic moment is always a proportion of the absolute moment. This proportion, when multiplied by 100, becomes the percentage that the algebraic moment is of its maximum.

$$100\ \mathrm{Sk}/\mathrm{Sk}_{\max} = 100\ \Sigma x^3/\Sigma\,|x|^3 = \%\ \mathrm{Sk} = \text{“percentage skewness”}$$
$$= \frac{100\ \Sigma x^3}{M^3 N^3} \text{ if } N \text{ is large.§} \qquad (5)$$

As N gets large, terms of the order of $1/N^2$ may be neglected. Dividing the last equation by N yields the monopoly skewness as the mean cubed times N squared:

$$\frac{\Sigma x^3}{N} = \frac{(\Sigma X)^3}{N^3} N^2 = M^3 N^2 = {}^{\mathrm{Sk}}\text{monopoly (for } N \text{ large).}$$

$$\%\ \mathrm{Sk} = 100\ \Sigma \frac{x^3}{N}\Big/ M^3 N^2 = \frac{100\ \Sigma x^3}{M^3 N^3} = \text{“percentage skewness” (for } N \text{ large).} \qquad (6)$$

The interpretation of kurtosis as flat-toppedness or peakedness of a distribution curve is useful in unimodal distributions. But with wider variation kurtosis measures the tendency of an unimodal distribution to split up into two distributions at opposite extremes and so to become a perfectly bimodal distribution (when % Ku = 100). Percentage kurtosis is thus easily interpreted by the layman as the percentage of a population's transition from one "middle-of-the-road" camp into two opposite camps. As it increases, it measures the extent of any split of a unified group into two antagonistic groups in respect to x. As it decreases, it measures the merging of two separate groups into one group. Maximal unity is at % Ku = 0, maximal duality is at % Ku = 100.

The fourth moment about the mean $\left(\frac{\Sigma x^4}{N} = \mathrm{Ku} = \text{kurtosis}\right)$ can be expressed as a percentage of its maximum by using standard deviation units and inverting the moment. Since the fourth moment in sigma units varies from unity to infinity, its inverse varies from unity to zero. Multiplied by 100, this has been called the index of nonconformity, or dispersion from some norm taken as origin.‖ If this norm is the mean, the nonconformity index can only reach its maximum of 100 per cent when all the cases are balanced at both extremes as far away from that mean as possible.

$$\%\ \mathrm{Ku} = 100N/\Sigma \frac{(x)^4}{\sigma^4} = 100(\Sigma x^2)^2/N\Sigma x^4 = 100N\sigma^4/\Sigma x^4 =$$
"percentage kurtosis," an index of nonconformity, or cleavage. (7)

These moments in percentage units do not change their significance from sampling as given by their standard errors from their usual formulas. But their translation into percentage terms should help greatly to improve communication—difficult at best—between technicians and laymen.

In summary, the statistical moments are expressible as per cents of their maximums, as follows:

Relative frequencies are proportions of the zeroth moment, as it is unity by definition.

The percentage mean, or first moment, has little usefulness except when it has a finite or natural maximum. Its index is the complement of the inverse coefficient of variation.

The percentage variance measures the observed variance as a per cent of its maximum, which is the case of monopoly. Its index is the ratio of the observed variance to the monopoly variance.

The percentage skewness measures the observed degree of asymmetry as a percentage of maximal asymmetry which is the L-shaped distribution. Its index is the ratio of the algebraic third moment about the mean to the absolute third moment.

The percentage kurtosis measures the unity of the distribution as a percentage varying from maximal unimodality to maximal bimodality. Its index is the fourth moment divided into the square of the variance.

§ Derivation of the third moment about the mean in a monopoly:

‖ For the conformity index, see S. C. Dodd. *Science*, **110**, 233 (1949).

Case	Raw Score (X)	Mean ($\Sigma X/N$)	Deviation from Mean ($X - \Sigma X/N = x$)	Frequency (f)	Deviation Cubed Times Frequency (fx^3)
Monopolist	ΣX	$\Sigma X/N$	$\Sigma X - \Sigma X/N = \Sigma X(1 - 1/N)$	1	$\Sigma^3 X\,(1 - 3/N + 3/N^2 - 1/N^3)$
All others	0	$\Sigma X/N$	$0 - \Sigma X/N$	$N - 1$	$+ \Sigma^3 X \frac{(-N+1)}{N^3}$
Total					$\Sigma^3 X\,(1 - 3/N + 2/N^2) = \Sigma x^3$

Feed the Soil!*

F. LYLE WYND

A native of the corn belt, the author took his Ph.D. in plant biochemistry at Washington University in 1933. He has taught at the universities of Missouri and Illinois and since 1945 has been in the Department of Botany and Plant Pathology at Michigan State College, where he is a research professor.

THERE is an ever-widening rift between the policies of the fertilizer industry and those based on the results of research in the fundamental properties of soil. This rift can no longer be ignored by the fertilizer industry, for already it has lessened significantly the effectiveness of the industry as a partner in the general agricultural program.

Somewhere along the line of its industrial progress the fertilizer industry has missed the boat, so to speak, and it is now pursuing a course of its own plotting that is leading it into conflict with the results of scientific research. The more the soil scientist learns about the general problem of fertility, the more intense becomes the advertising propaganda of the fertilizer industry to preserve its own original approach to the problem. Unfortunately for everyone concerned, there are now two schools of thought concerning the proper use of fertilizer, and the resultant confusion in the mind of the farmer must inevitably be injurious not only to him but to the fertilizer industry as well. It is therefore of the greatest importance that the basis for his confusion be carefully examined. No one would deny that the progress of agriculture is essential for the national welfare, and that this progress is impossible without the cooperation of the fertilizer industry. This cooperative effort depends on the well-placed confidence of the farmer in the organizations that sell him fertilizer, and such confidence is incompatible with the present confusion and uncertainty.

The nucleus of the problem is the concept of what fertilizer really is, how it should be used, and for what purpose. Before embarking on our discussion, perhaps we should state in precise terms just what the two conflicting concepts are, so that the arguments supporting each will not add to the confusion.

Soil scientists regard soil as a complex, ever-changing, biological equilibrium. The products of this dynamic complex furnish food for plants. This means that fertilizer practice should be designed to further the desirable aspects of the soil's own activities, so that it, in turn, will be able to feed the plant. Fertilizer, then, must be regarded as *soil food.* The opposing concept appears to regard the soil merely as a physical matrix that supports the plant and that contains soluble materials used as food by the plant. This means that fertilizer practice is designed to feed the plant directly in accordance with its specific needs. It follows, in this case, that fertilizer must be regarded as *plant food.*

We believe that the concept of fertilizer as *specific* plant food is not entirely correct, and that this misconception has been popularized by the advertising program of the fertilizer industry. We have no quarrel with the fertilizer industry itself. Our only intention is to call attention to an error that has resulted in so much harm to the industry and to the farmer. It should be emphasized that we are concerned in this discussion only with the problems of "plant food" as the term is currently used by the fertilizer industry.

Plant food is a simple term, unadorned with subtle connotations. It seems impossible that the use of such a direct, matter-of-fact pair of words could be so twisted and perverted that they mean nothing recognizable. How hopeless it is for the farmer, under these circumstances to know what plant food really is, and how he may conserve it and augment its supply in the life-giving soils of America.

The fertilizer industry, anxious to profit from the comparatively recent advances in agricultural research, has so simplified its explanation of soil fertility that many intelligent farmers wonder why so many soil scientists are still engaged in research at the public expense, when any fertilizer salesman will diagnose soil problems gratis and sell him the necessary "plant food," all neatly packaged and labeled.

Soil scientists and plant physiologists, however, will tell the farmer that plant foods are components

* Based on an address presented at the annual convention of the Plant Food Producers of Ontario, at Tadoussac, Quebec, June 1950.

of the colloidal and dynamic biological complex of the soil, and that plants obtain their foods from this complex organization, in a manner of speaking, only by permission of the soil. The description of this intricate process cannot be condensed into a few words, for there is no way that the problem of soil fertility can be stated simply. How tempting it is, therefore, for the fertilizer industry to ignore the complex nature of soil fertility and to dismiss its intricacies as academic verbiage. Economic necessity forces the industry to so oversimplify its discussions with farmers that the true nature of the problem is submerged and forgotten. The salesman, staring at the soil and kicking a clod with a knowing air, often explains soil fertility to the farmer in terms of something in a sack that he will gladly sell for so much per pound. The farmer willingly agrees, for he wishes to be scientific and up to date, and he may wonder why, after all, plant food is not equivalent in concept to animal food—for certainly food is food, no matter how much the scientists may try to confuse him.

What the farmer often does not realize is that he is the victim of an error, and that he has fallen into the seductive arms of modern advertising. The error sometimes is forced upon him in ignorance, but many examples of advertising propaganda could be collected that are so obviously absurd that one is forced to believe that the farmer is the intended victim of a deliberate campaign.

Lest I be accused of confusing the problem even further for the farmer, let me hasten to explain in some detail what the problem of plant food really involves, and to justify to the fertilizer industry the accusation that it has limited itself as an industry in its anxiety to profit from an advertising policy based on error. It is unthinkable that the fertilizer industry should exist except as a service to agriculture and, since its future prosperity depends upon the augmentation of the effectiveness of this service, it is of the greatest importance that current misconceptions be corrected.

First let us consider how the scientists regard plant food, and how they believe plants obtain it from the soil. We shall then be able to dismiss false conceptions from our mind, and to proceed according to a reasonable plan to preserve the fertility of our soils and to restore impoverished soils to their original vitality. The timorous salesman need not be fearful for the financial future of his family, because the proper use of fertilizer frequently involves quantities far beyond anything indicated by the pretty colors on his porcelain spot plate or by his astonishingly simple, but incredibly inaccurate, "quick tests."

Our discussion will be based on two fundamental characteristics of soils. The first is that the soil is biologically alive and, second, that its colloidal properties govern the release of food to the plant. We might say, in other words, that the nutritional aspects of soil fertility depend on the activities of living microorganisms and on the electrical properties of its nonliving, colloidal components. The proper use of fertilizers is based on the effects of artificially added chemicals on these two fundamental, related, and mutually dependent characteristics of soil. Soil fertility is governed by the proper magnitude of these characteristics, as well as by their proper relationship to each other.

Our first task will be the consideration of the organic life in the soil, because all that the soil is or may become depends upon it. A fertile soil is not merely a layer of tiny fragments of rock and clay, nor a dead mass of inert fragments mechanically supporting plants while their roots explore the crevices for water and soluble plant food. If soil were this lifeless thing, we could truly speak of plant food as something in a sack which costs a trifling amount per pound.

Soil is alive, truly, actually, and literally alive. Soil is not soil until it is alive, and no amount of chemical plant food mixed with dead, finely ground rock particles would produce the equivalent of a productive soil. If soil were a lifeless thing, we would never concern ourselves with erosion, or leaching, or with any phase of soil conservation, as long, of course, as the physical texture of the mass permitted seeds to be planted and roots to explore. "Plant food" would solve all the farmer's problems, and he could boast, as he frequently foolishly does, that the soil on his farm is twenty feet deep. If he had eyes that could see the soil in all of its microscopic and submicroscopic details, he would see that the living portion of it is only a few inches deep, and he would shudder when he saw muddy rivulets carrying it away.

I am sure that no one really believes in such an absurd concept of soil fertility. Obviously, then, soil must be something more than a mixture of fine particles and chemical plant food. This "something" is the result of the living nature of soil, and it is the real basis of permanent soil fertility. It must be preserved if fertility is to be preserved and it must be augmented in impoverished soils if they are to regain their original productive state. Soil conservation is concerned specifically with the preservation of this property. If it be preserved, all is well. If it be not preserved, no amount of "plant food" dispensed from sacks will itself remedy the loss.

Since it is impossible to discuss briefly the importance of organic matter in soil, I shall mention here only those aspects of the whole problem of organic matter that pertain to our argument in support of a proper use of fertilizer. Three somewhat similar phrases have already crept into our discussion; they should not be considered as synonymous, because they are subtly but importantly different. *Life* in the soil refers literally to microscopic living organisms. The *organic matter* of the soil, in the sense of chemical components determined in the laboratory, is not living at all, but represents a complex mass of materials derived from dead organisms or produced by them. The life in the soil may be observed with suitable bacteriological techniques, the *something* which results from the activities of the living organisms can scarcely be measured or defined except in terms of crop response. The organic matter is visible to the eye since it is responsible for the color of the soil. In its absence most soils would be red or white-gray.

We need not pursue the discussion of the importance of the complex biological equilibrium to soil fertility except to emphasize that if the living microscopic organisms in the soil are thriving properly, all desirable aspects of subsequent organic soil equilibria will occur automatically.

Many people erroneously believe that chemical plant food, plus dead organic matter, constitutes fertility. The great value of preserving the topsoil from loss by erosion and of increasing the amount of organic matter in it by plowing under green cover crops is well known, but a soil was never fertile because it contained inorganic plant food and dead organic matter. The life of the soil, with which we are now primarily concerned, is not synonymous with the organic matter, which may be so easily measured in the laboratory.

If we admit that the living microorganisms in the soil must thrive in order to maintain and augment fertility, then we must also admit that the maintenance of this living population is of great importance. These organisms require food, and its supply must be maintained, often by the use of large quantities of fertilizer.

The total amount of living organisms in the soil is unbelievably large. For example, as many as 1 million protozoans, 800,000 algae, 1 million fungi, and 20 million bacteria may inhabit a single gram of soil. Since an acre plow slice of soil weighs about 2 million pounds, or about 1 billion grams, we have only to multiply the already prodigious number of organisms per gram by 1 billion in order to estimate the number present in one acre plow slice of soil. The figures quoted are conservative estimates but certainly we may agree that the total number is beyond our ability to comprehend. The actual total weight of the invisible living organisms in an acre plow slice of soil is from 10,000 to 50,000 pounds.

If we could see these living organisms with our eyes the soil would appear to be a seething mass of life, concentrated in the upper few inches of the soil profile. The supply of nitrogen, phosphorus, and sulfur, and of many other plant foods as well, is completely dependent on the metabolic cycles undergone by these microorganisms. How do we provide for their welfare, and what consideration should they receive in routine fertilizer practice? Before we attempt to answer this question, another aspect of soil should be described.

The most important component of the *inorganic*, mineral structure of soil is clay. This does not mean that the more clay the better the soil, but it does mean that clay must constitute a reasonable fraction of the soil before it can become fertile. The amount of clay present in soil varies between wide limits, and is one of the important criteria used in the current system of soil classification; this itself indicates the importance of this component. A discussion of the important physical properties of clay lies beyond the scope of this paper, but we may briefly describe the properties that influence the nutrition of plants and the effectiveness of fertilizer practices.

Clay is not composed of tiny particles of rock. The finely structured material ground from the rocks by glaciers—commonly found in many parts of the country—is not clay. Clay is a chemical product of the weathering effects of moisture and carbon dioxide on silicate minerals, and is composed of tiny crystals, too small to be seen even with a miscroscope. A clay crystal is less than 2 μ in diameter, and the diameter of 12,500 clay particles of the largest possible size would be only 1 inch. The particles are so small that they possess special properties, and are therefore said to be colloidal.

Clay is comparatively inert chemically, and for all practical agricultural purposes, weathered clay crystals lie in the soil for long periods without undergoing pronounced chemical change. The importance of clay to the general problem of fertility depends on the fact that the surfaces of its crystals bear a permanent and negative electrical charge, which attracts positively charged ions into its immediate vicinity and holds them there until they are removed by living organisms, or by the physi-

cal competition of other positively charged ions.

The so-called available fractions of calcium, magnesium, potassium, iron, zinc, copper, manganese, and cobalt are held in the soil by electrical attractions to the surfaces of the clay crystals. This ability of a soil to retain positively charged ions by virtue of the electrical properties of its colloids is termed its base exchange capacity.

Since all soluble ions in the soil are free to leach downward unless they are held by the electrical forces of the soil colloids, it is apparent that the magnitude of the base exchange capacity determines how much calcium, magnesium, potassium, iron, etc., may be retained by the soil—or, in other words, how many of these positively charged nutritional ions may be stored in the available state. The base exchange capacity defines the ultimate limit of soil fertility, at least in respect to the positively charged ions. The ions retained in the soil by virtue of its base exchange capacity are called replaceable bases. It is useless to attempt to exceed permanently the limit of fertility by the addition of more and more of the conventional types of inorganic plant food.

Now is the time to mention the importance of organic matter, because the dead, complex organic products produced by the living organisms in the soil also possess a base exchange capacity. Any increase in the proper type of organic matter increases the base exchange capacity of the soil, and consequently the ability of the soil to retain and accumulate the positively charged plant nutrients also increases. If the living organisms in the soil prosper in the correct manner and in the presence of the proper nutrients, the residue of organic matter also accumulates, and the potential fertility increases accordingly. We can now understand why organic matter by itself is not equivalent to fertility. Its presence, however, is essential in order that the soil may retain and accumulate positively charged plant nutrients as they are released by weathering, bacterial activity, and the addition of fertilizer.

It is not enough, however, that the colloidal surfaces of the clay and organic colloids be saturated with available plant food, for these foods are held by electrical forces. The nutritionally important replaceable bases on the surfaces of the soil colloids are subjected to a tug of war between the electrical forces on the soil colloids and those on the absorbing roots, and they must go where the attractive force is greater.

The apparent availability, or the ease with which the plant wins this tug of war, depends on how firmly the replaceable bases are held on the surfaces of the soil colloids. Soil physicists have shown that the geometric spacing of the replaceable bases in the vicinity of the soil's colloidal surfaces may be modified so that ions differ in the mechanical problem of their escape from the surface of the clay crystals. We may take their word for it and merely state that the mechanical problem of the escape of a nutrient ion from the vicinity of a colloidal surface in the soil is less if the ions are clustered in certain ratios to each other. For example, it is generally agreed that about 80 per cent of the total replaceable bases should be calcium, about 18 per cent magnesium, and about 2 per cent potassium. These percentages are not critical in magnitude, but the ratio of the amounts of the different ions adsorbed on the soil colloids is an important factor influencing their true availablity to the plant.

The somewhat intricate relationships among colloidal surfaces, electrical forces, base exchange capacity, and replaceable bases furnish the foundation for current liming practices. Soil acidity has not been mentioned because it is only remotely related to our problem, but perhaps unnecessary confusion will be prevented by stating that an acid soil needs lime. The function of lime is not to lessen the acidity itself, but to replace hydrogen ions adsorbed on the soil colloids and to disturb the electrical and geometrical symmetry of the ions adsorbed on the soil colloids, and thus facilitate mechanically their migration to the electrically charged surface of the root. The effect of lime added to an acid soil is therefore an increase in the availability of other plant nutrients. Since we have mentioned acidity, we should add that, although an acid soil reaction indicates the need for lime, it cannot indicate the required amount. The correct amount can be determined by first determining the base exchange capacity and then measuring the percentage of that capacity not already satisfied by replaceable bases. If all else is equal, the lime requirement of a soil is exactly determined by the unsaturated base exchange capacity, and it is only indirectly related to the pH value of the soil solution. The currently used laboratory tests for the lime requirement frequently are based on the determination of the replaceable hydrogen adsorbed on the soil colloids.

The two phases of the nutritional complex of the soil that govern fertility are both influenced by the addition of fertilizer. Our discussion so far has been based entirely on the significance of fertilizer as soil food, and we have not yet mentioned plant food at all.

The biological and colloidal requirements of the soil for the proper chemical substances are determined by the state of the soil itself, and they are quite independent of the specific nutritional requirements of individual crops. In many instances, this need for soil food is comparatively great, and it is especially great if the soil has been allowed to lose its fertility by bad management, or if it has been ruined by surface erosion. If we think of fertility in the long-time sense, we must first think of the biological and colloidal requirements of the soil itself and only secondarily of the nutritional needs of specific crops.

Since the very agricultural nature of soil depends upon its biological and colloidal state, one wonders that any productive soils remain after many years of abuse and mismanagement. But nature wisely intervenes to save man from the results of his own stupidity. Unfortunately for the avaricious farmer, but luckily for civilization, the living microorganisms in the soil cannot be overlooked. Nature has decreed that they shall have first choice of the available nutrients, and it is lucky for us that the fundamental soil properties are thus preserved. The actual fertility may almost disappear, but the basis for its rejuvenation is preserved, since the crop plant may take only what is permitted by the bacteria. There is no successful way to feed the plant without first feeding the life in the soil. It is amusing to a soil scientist, and he chuckles inwardly with pedantic satisfaction, when he hears a farmer's lament that he has added so many pounds of phosphate per acre and obtained no significant increase in the yield of corn. It is lucky for all of us that he was unavoidably feeding bacteria and thereby preserving some of the essential properties of the soil for a future and more intelligent master. We can all give three cheers for phosphate fixation, which plagues the farmer, but which saves us from starvation.

There is a deplorable and foolish tendency apparent in current agricultural research to avoid the necessity of feeding the soil, and to feed the plant directly. Obviously this is an economy measure—an attempt to grow the maximum crop with the least amount of fertilizer. Even if such a disturbance of nature's equilibrium were successful, the most that could be said for it would be that a particular crop was produced in a particular season with a minimum expense for fertilizer. Obviously, the permanent basis of fertility would not be increased, nor would any phase of soil conservation and rejuvenation be improved.

Any concept of fertilizer as plant food, to the exclusion of its importance as a soil food, is based on the belief that the plant can be fed directly without first feeding the soil. Such a niggardly practice is uneconomical for the farmer in the long run. His soils will unavoidably deteriorate if he practices it over any considerable period of time, and this will force him to use more plant food, with still greater deterioration of the soil. Such a practice is contrary to a sound agriculture, contrary to the ideals of the conservation of fertility, and therefore antisocial and unpatriotic.

Current attempts to feed the plant without first feeding the soil include the so-called placement studies of fertilizer. If fertilizer is so placed in the soil that the roots of the crop come into maximum contact with it, and the soil comes into minimum contact with it, the use of the fertilizer is said to be more efficient. The correctness of this conclusion depends on what we mean by efficient. It is efficient, in that the soil is temporarily outwitted, and the plants are fed more or less directly. It is inefficient in the sense that the biological phase of the soil, the all-important basis of permanent fertility, has not only been ignored, but has been diminished.

These deplorable attempts to outwit Mother Nature are of various types, but they are all based on a concentration of the fertility in the zone of maximum growth of the crop roots. Drilling in fertilizer close to the seed row, placing it in a band below the seed row, and top dressing are common examples.

The long-continued use of mineral fertilizers as plant rather than as soil food always leads ultimately to a decline in productivity. Because this decline is extreme in some instances, there is an increasing resentment toward the use of mineral fertilizers among a group of fanatic faddists. Unfortunately for the fertilizer industry, the argument of these fanatics is based on facts, making their absurd conclusions embarrassingly difficult to attack. The most dangerous of all fanatics, whether political, scientific, or religious, are those who base their propaganda on facts, but who lack enough facts to support a correct conclusion.

Scarcely a month passes that some tract of the Organic Gardening Society does not come to my desk. Its ideal apparently is to banish inorganic fertilizer from the earth as an evil influence on mankind. It argues that there is something associated with biological fermentation in the soil that far transcends the value of fertilizer, and that the continued use of inorganic fertilizer is ruinous. Alas, the society is correct in its major premise! After all, the entire thesis of our own discussion is the necessity for feeding the living organisms in

the soil. It is a tragedy, however, that this society does not know that the proper use of fertilizer as a soil food is the true solution of the problem.

The students of fertilizer placement and the faddists are not the only groups who ignore the possibilities of fertilizer as a soil food. The fertilizer industry itself has been the greatest offender in its advertising campaign pushing standard mixed fertilizers. If a mixed fertilizer is based on true soil and crop requirements, then its application in a mixture is economical and sound. If, however, the composition of a mixed fertilizer is based only on a supposed food requirement of a specific crop, as if this were as precise as a baby's formula, it is not agriculturally sound, the farmer who uses it will not receive the maximum return for his investment, and the national program of fertility conservation will not have been served.

I do not pretend to know the bases of the many different mixed fertilizers on the market. I do know, however, that farmers generally assume that the ratios of nutrients represent the plant food requirements of certain crops, just as specific as the food requirements of laying hens. Even if the need of the soil for food were not significant, it is doubtful if plant food could be reduced to fixed fertilizer formulas equally effective on different soil types in the same area—to say nothing of the problems of different areas and different seasons.

The use of mixed fertilizers of standard composition has cost the farmers of the United States many millions of dollars more than necessary. If enough of any one component is added to a field in a mixed fertilizer, it is almost certain that an unnecessary amount or an inadequate amount of some other component is added. The financial loss incurred and the potential profit lost contribute to the high cost of fertilizer and tend to lessen its use. There are extensive areas where the amounts of replaceable potassium in the soils far exceed the maximum requirements of the soils and crops. The soils of some regions are so diverse, even within a limited area, that many fields do not require potassium, although near-by fields may exhibit a potassium requirement of considerable magnitude. The selling of a mixed fertilizer to farmers in these areas without adequate soil tests is economically unsound. Of course the response of the farmer's crop to the nitrogen alone may more than repay him the total cost of the mixed fertilizer, but this does not justify selling him potassium when he does not need it.

Now we see that there are two different concepts of the proper use of fertilizers: One that fertilizer is a soil food, necessary in order that the biological and colloidal equilibria in the soil can feed the plant in the manner intended by nature; and the other that fertilizer is a plant food, necessary to feed the plant directly and ignoring the biological and colloidal requirements of the soil. The use of fertilizer as a soil food conserves and augments fertility, but its use strictly as a plant food does not conserve, but actually lessens fertility if pursued over an extended period.

The future, in the long-term sense, of agriculture, and the prosperity of the fertilizer industry, depend upon which of these concepts dominates the farmer's use of fertilizers, and upon how effective are his efforts to conserve and augment the fertility of his soils.

The association of the problem of the proper use of fertilizer with that of the conservation of soil fertility is inescapable because they are identical over any considerable period of time. Only over short periods may soil fertility and soil conservation be considered separate problems. If a farmer plans to produce only three or four crops and then abscond to the city with his ill-gotten gains, he may think only in terms of fertilizers as plant food. If he is truly a farmer and not ashamed of it, he will not plan his life to begin when he escapes contact with the soil. Much more likely, his greatest concern will be the preservation of that fragment of his country that a democratic society permits him to call his own, which will constitute a legacy to his children. Such a farmer must think of soil fertility as synonymous with soil conservation.

Which of these farmers is the best business risk for the fertilizer industry? On which of them does the future of the commercial production of fertilizers depend?

The easiest man to sell to, because he is the most gullible, is the one who steals the life from his own soils, and runs to the city with his plunder. He is the one who asks, "What must I buy from your sack to grow a crop of corn?" He thinks only in terms of a temporary supply of plant food, for he wishes only to grow a crop as cheaply as possible. To him the soil is an inert support for his plants. He is not concerned with augmenting the living quality of the soil he cultivates so ruthlessly. His crop, which he thinks of as a "cash crop," requires twenty pounds of phosphorus; therefore he will add twenty pounds of phosphorus. Anyone could sell it to him, for the sales argument obviously is correct from his narrow viewpoint. The pitiful part of the transaction is that both the farmer and the salesman fondly imagine that

they have been quite scientific in the matter. There are thousands upon thousands of acres of once-productive soils in the United States that are now abandoned, for even a sharecropper cannot eke out his unhappy existence from soils that have been nearly killed by their past masters. The country becomes smaller year by year if measured by its total productive area, as more and more acres die because their owners did not know—or did not care—that soils must remain alive if they were to make them rich.

The problem of the enlightened farmer is to maintain the living nature of his soils. If he accomplishes this, his soils will be productive. The maintenance of this productivity requires thought and proper management, but it should comfort the representatives of the fertilizer industry to know that it also requires fertilizer, frequently in quantities larger than could be justified on the basis of its use as plant food.

The over-all effect of the use of fertilizer as a soil food would establish the fertilizer industry as a permanent and essential factor in agricultural prosperity, and as a force of the greatest importance in the program of soil fertility conservation in the nation.

THE CANCER CRUSADE

At this time of year the American Cancer Society seeks contributions of funds to continue its Cancer Crusade—a fight against a disease that strikes one in every five of our population. It has reached this pinnacle of deadliness not only because other diseases with high mortality rates have fallen victim to the attacks of research, public education, and enlighten therapy somewhat more readily than cancer, but also because our inhibited society felt that some sort of mystic and disreputable taboo attended the dread disease. Until recent years few victims died officially of cancer.

At this time of year, also, it is customary for news writers to dust off the horrors of cancer, dig up a few new statistics and adjectives, and attempt to frighten the public into supporting the Cancer Crusade with donations. The Editors of The Scientific Monthly are confident that the readers of this journal, as scientists, do not need to be frightened, and that their knowledge of the scientific method, their intelligence about insurance, their own professional concern about the applications of research methods to scientific problems which affect so large a segment of the human race—and perhaps, statistically, themselves—will suffice to make them generous in their support of the Cancer Crusade.

The practice of medicine has long been an art, as much as it has been a science, primarily because the human race is so constituted as to demand that its ego be flattered while its physical ills are receiving scientific care. Now that cancer, like venereal disease, can be named, it can be attacked with all the ingenuity of research that financial resources can provide. The American Cancer Society has moved in on the problem along three fronts: relief of sufferers, education of those who may be sufferers, and research to cure and to arrest the disease.

The American Cancer Society will be grateful to receive your contribution at 47 Beaver St., New York 4.

When Spring's First Heralds Hum

SPRING LAUGHTER IS OFF STAGE

The skies have lost their worried cast,
Their leaden hue;
Though gray, they have a pussy-willow touch,
A splash of water-color blue.

The earth, as if unused to laughter,
Seems a little shy
To come right out with it;
And so it chuckles first,
Until the down-trickling melt
Swells to a throaty rush.

Green fingered darts—
  the squills of hyacinths,
  the slender sheaths of crocuses,
  the tulips' pudgy buds—
Break through the morning crusts of ice.
A few, incongruous,
Like lonely palms upon a desert isle
Stand on snow sandbars,
Fast-fading in the mounting tides of sun.

JOSEPH HIRSH

Trumbull, Connecticut

FALCON

High above, the peregrine,
hanging on wind over the world,
sailed free, held
by an unforgetting fealty.

Leaped from a leathern arm,
the wonder of unhooded flight—
a sight of sunlight
blinding as the night.

MARTIN S. DWORKIN

New York

RED-WINGED BLACKBIRD

The red-winged blackbird and his whistle:
An airborne elemental missile—
Arrowed answer without name,
Small dark question lit with flame.

HORTENSE ROBERTA ROBERTS

Whittier, California

GINKGO BILOBA

Dieses Baums Blatt, der von Osten
Meinem Garten anvertraut,
Gibt geheimen Sinn zu kosten,
Wie's den Wissenden erbaut.

Ist es Ein lebendig Wesen,
Das sich in sich selbst getrennt?
Sind es zwei, die sich erlesen,
Dass man sie als Eines kennt?

Solche Frage zu erwidern,
Fand ich wohl den rechten Sinn:
Fühlst du nicht an meinen Liedern,
Das ich Eins und doppelt bin?

GOETHE

Ginkgo leaf that from the East
Traveled to my garden's gate,
Vessel of some unknown quest
For the wise to contemplate.

Has One being once divided
To be two within its own?
Are there two that have decided
To be known as One alone?

That the answer may reveal it,
Of this heart in me for you:
Listen to my song and feel it,
How I am both One and two.

(TRANSLATED BY ROBERT BLOCH)

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SCIENCE ON THE MARCH

AN EXAMPLE OF HOW DAMS DESTROY VALUABLE SCIENTIFIC RECORDS

NEARLY everyone is saddened by the destruction of beauty—whether that beauty be in the form of a gaily colored bird, or a masterpiece of scenery such as the Grand Canyon, or some architectural gem or other form of human creation. Destruction of historic monuments, landscapes, and other priceless values in times of warfare is often excused because of the immediate exigency of the circumstances. Their needless destruction at other times, however, should be a cause for alarm and for spontaneous action on the part of the citizenry. The question of allowing the government to spend millions of dollars, at the suggestion of the Corps of Engineers, to erect huge dams, especially where the usefulness of such dams is seriously in doubt, is a case in point.

The permanent flooding by large dams of immense expanses of beautiful landscape, together with the attendant destruction of valuable forests, animal and plant habitats, good agricultural land, historic archaeological sites, or unique caves and springs and the peculiar forms of life they harbor, seems small compensation indeed for what follows in the form of speed boats, neon-lighted resorts, other commercial developments, and eventual silting of the reservoir with muddy water. The reason given by the Corps of Engineers for the construction of the dams (if not for hydroelectric and recreational purposes) is usually that they are needed for flood control, but soil conservationists, forestry experts, and many other scientists seriously question the value of building big, expensive dams for flood control, insisting that the same result, with much less destruction of the landscape, wildlife, and land values, could be accomplished by better soil conservation practices, reforestation, and small check dams at the headwaters, rather than lower reaches, of streams. The construction of dams over many sections of the United States seems to have become a monomania with the Corps of Engineers.

It is no wonder, then, that scientists throughout the country are seriously concerned about such grandiose projects, the real need for which has not been subjected to scientific proof. The fact that some of the dams in the United States have, after twenty-five years or so, begun to fill up with silt and muddy sediment, making them useless and creating the need for a new dam site, illustrates the short life and temporary nature of the work. In fact, the projects for damming various areas are in themselves experiments and are part of a pork-barrel enterprise often associated with a desire for political gain and the development of prestige and other forms of human selfishness.

The field laboratory and workshop—yes, the very habitats with which biologists work—are being threatened as never before. It is not just the loss of a nesting site for some species of bird here, a loss of a woodland there, or of an archaeological site of historic importance or a geological stratum; it is the multiplication of all these in many places and, in addition, the complete and final destruction of actual records. The knowledge of the geographical station and distribution of bird, mammal, and reptile species in the United States is fairly complete, but that of other forms of life, particularly the distribution of species of plants, is still inadequate. Botanists, for example, are constantly adding to the knowledge of the exact northern, southern, western, and eastern limits of geographic distribution of a given plant species by going into the field and collecting specimens that serve as records for the locality where that species occurs in nature. A great impetus to the discovery of new stations for plants, thus adding to the complete picture of plant geography, has been given in the past few decades by new and improved roads that facilitate travel by automobile. Unfortunately, there are so many previously unexplored areas that the field biologist cannot adequately cover all the counties or townships of a given state even in a lifetime. Many areas face real-estate or other commercial development and resultant destruction of natural habitats. Burning and clearing of land for agriculture, stream pollution, laying of pipelines and roads—these and other forms of development further reduce the available habitats.

The completion of the Bull Shoals Dam project by the Corps of Engineers has dealt a deathblow to at least one species of plant and completely eliminated it from the flora of Missouri. The plant is the French mulberry (*Callicarpa americana*). This shrub of the verbena family (Verbenaceae) has unusually beautiful bright-purple or violet-blue fruits of a color rarely found in nature. Indeed, the color is so striking that the shrub has been named beauty-berry. It is fairly common in the Southern United States,

ranging from Florida west to Texas and north to Maryland, Tennessee, Arkansas, and Oklahoma. For many years botanists have been searching for stations where it might be growing wild in Missouri. This is natural, since it grows commonly in Arkansas and has been found not far from the Missouri line. Missouri is a large state, and exploration of previously unbotanized areas continues to yield many new records of species not known heretofore from the state, as well as new geographical boundaries of those species, thus providing a student of plant geography, systematic botany, ecology, and evolution with a more detailed picture of the exact distribution of a particular plant species.

On a fall day in September 1949, I was engaged in a plant survey along a portion of the White River in Taney County, in southern Missouri, just within the borders of the state and not far from the Arkansas line. My purpose was to collect and record samples of the different kinds of plants that occurred along this stretch of the river, since it was destined for destruction by the eventual impoundment of the Bull Shoals Dam. The specimens collected were to serve as a record, therefore, of the natural vegetation in this locality before the waters of the dam obliterated it. From Browns Ferry I was making my way downstream toward Cedar Hol along rocky limestone wooded slopes facing south into Arkansas. I had not found anything very significant, but suddenly my attention was drawn to a mass of brilliant purple color, and, as I approached the spot, I could not at first believe my eyes. For here, at last, were plants with the bright-purple fruits that characterize the French mulberry. The small round fruits shone exquisitely in the bright sun and added a sparkle of vivid beauty to the surrounding woodland. Numerous bushes occurred on the wooded slopes twenty to twenty-five feet above the surface of the river. Climbing over the rocky slopes above and beyond this point, I found many more plants scattered nearly to the lowest line of limestone bluffs above, approximately sixty feet in vertical height above the river.

The joy I felt upon the discovery of this plant, the first time after all these years it had been found in Missouri, and over establishing a new northwestern limit of geographical distribution for the species, was quickly dispelled by the gloomy thought that all the plants here and the species itself would become exterminated from the flora of the state when the waters of Bull Shoals Dam entered the area. For, although some of the plants

Habitat of *Callicarpa americana* in Taney County, Missouri. Lower half of wooded slopes along White River in left half of photograph. Scene taken before Bull Shoals Dam had flooded area. The lower half of these same slopes is now part of the lake created by damming White River.

had found it possible to grow on the slopes as much as sixty feet above the water level of 1949, the future impoundment of Bull Shoals would not only completely cover this sixty feet, but an additional fifty to sixty feet besides. The water, in other words, would be raised well up on the face of the high south-facing limestone bluffs that tower far above the last upper stand of *Callicarpa* bushes.

In May 1951 I revisited the spot. The beautyberry was still there, safe, a lovely part of the natural scene, but during the latter part of October 1951 what had been quiet, restful stretches of river—the haunts of birds and other animals—became an area of wild destruction. The waters of the Bull Shoals Dam were beginning to rise on White River and its tributaries. Shorelines were becoming permanently flooded. Trees and shrubs of all kinds were being ruthlessly cut down and burned, from the old level of the stream to the upper new level of the impounded water. An inferno of flames and smoke passed over the southwestern Ozarks, and with it went the first and last stands of the French mulberry discovered in Missouri.

I thought sadly of many similar examples of destruction that are taking place as a result of such big impoundments. Little does the public that sees these vast man-created lakes for the first time realize the fate of the natural landscape and the destruction of various wildlife habitats—in short, the loss of primitive America, and a part of our natural heritage and resources.

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THE ARISTOCRAT AMONGST THE ANIMALS*

ALTHOUGH dated 1950 this volume did not appear until mid-1951, a year after the symposium was held. It is the product of many minds. There are 28 essays grouped under seven headings, accompanied by nine chairmen's summaries. Each essay, essentially what the speaker presented at the symposium, is followed by a verbatim report of the discussion that followed the original presentation. That there was no reluctance on the part of the listeners to dispute with the authorities is amply evidenced in these discussions. Thus they usurp the usual function of a reviewer. The "Concluding Remarks of the Chairman" synthesize the ideas presented by the speakers, sometimes generously salted with the chairman's own ideas or with points made during the discussions.

The first day's papers revolved around the thesis "Population as a Unit of Study." Two of these, the first and second, by H. H. Stranskov and A. Buzzati-Traverso, respectively, discuss the peculiarities of human mating systems and population concentrations as they affect the flow of genes and genetic combinations. The third, by F. P. Thieme, discusses some of the problems that confront those who engage upon population surveys. For this he used experience gained while making such a survey in Puerto Rico. Marston Bates' summary of the day's papers is short and to the point. Many will enjoy his sly comment upon the geneticists' habit of creating new polysyllabic words.

The second session was devoted to the "Origins of the Human Stock." A. H. Schultz opened with an extended review of what we know of the differences and similarities among man and the catarrhine primates. Of necessity this is primarily a skeletal comparison. Perhaps the most important general conclusion of Dr. Schultz is that man and the anthropoids vary to the same degree. In the brief report of discussion that followed the paper F. M. A. Montagu raised an interesting point. He asked whether there were data on the variability of such external features as hair and eye color, etc. Apparently little is known of this, but among chimpanzees such surface phenomena as, say, skin color, is useful to segregate subspecies. The second paper was presented by G. G. Simpson and dealt with "Some Principles of Historical Biology Bearing on Human Origins." This is a neatly assembled summary of points from which to view current theories explaining the origin of man. Although Dr. Simpson has based his summary upon a vast knowledge of vertebrate paleontology and applied it to man, students of invertebrate evolution will find it equally applicable to their problems. His final caution against extrapolation, either backward or forward, should be pondered carefully by all builders of phylogenies. The final paper of the session was S. L. Washburn's "Analysis of Primate Evolution." In it he presented compactly many of the trends that have been observed in both the

* *Origin and Evolution of Man.* Cold Spring Harbor Symposia on Quantitative Biology, Vol. XV. 425 + xii pp. Illus. $7.00. Biological Laboratory, Cold Spring Harbor, N. Y. 1950.

skeletal and soft anatomy of the primates. In the discussion of this paper MacDowell pointed out that Washburn's suggestion that geneticists investigate the secondary effects of genes has been anticipated, and that among mice a genic situation has been discovered where the timing of the development of one group of bones, the ribs, affects the configuration of another bone, the sternum. The concluding remarks by W. W. Howells are introduced by a clear statement of the general impression given by the three papers: they ". . . agree strikingly in presenting a rejuvenated point of view on human-primate relationships . . ."

Washburn's suggestion made on the second day, that we recognize but two homonid genera, *Australopithicus* and *Homo,* coming as it did in the last session of the day, acted as a springboard into the third part of the symposium, "Classification of Fossil Men." Dr. Krogman held the reins for the papers presented by T. D. McCown, T. D. Stewart, and Ernst Mayr. McCown lead off with a discussion of Neanderthal man and concluded: "The structural differences between the majority of Neanderthal specimens and individuals or groups of *Homo sapiens* are real phenomena that indicate separateness of genetic history." The succeeding papers tend to dispute this contention. Stewart examined the "Earliest Claimed Representatives of *Homo sapiens.*" This was done in detail. His conclusion was that

> Like Dobzhansky, therefore, I can see no reason at present "to suppose that more than a single homonid species has existed on any time level in the Pleistocene." However, I arrive at this conclusion not as he does, from assumed evidence of hybridization, but from the available evidence of population variability.

Mayr approached the problem as a taxonomist. His discussion was most welcome. In no field of zoology—and man is an animal—have fundamental principles of taxonomy been ignored so frequently as in the study of man. His summary is succinct:

1. There is no conclusive evidence that more than one species of homonid has ever existed at a given time.
2. It is proposed to classify fossil and recent homonids into a single genus (*Homo*) with three species (*transvaalensis, erectus, sapiens.*)
3. The recognition of subspecies within the species facilitates classification.
4. The ecological versatility of man and his slowness in acquiring reproductive isolating mechanisms have prevented the breaking up of *Homo* into several species.

As might be expected, the discussion that followed divided the contributors into two classes: those with little taxonomic experience, including most of the older anthropologists, and those with taxonomic interests in some other field than man in addition to an interest in man. Throughout the entire nine-day symposium there is evidence of this split—man for man's sake vs. man as an animal. Krogman's synthesis of the three papers, with which he closed the session, terminates with this canny "final word of caution . . . that any taxonomic system cannot hope to provide for all the variables in the classification of fossil men. There must inevitably be a certain amount of arbitrariness in any reconstructive taxonomic framework." These words deserve a place above the worktable of every taxonomist!

The next three days were devoted to the "Genetic Analysis of Racial Traits." The first program was devoted to clinical aspects of the problem. J. A. Böök presented a detailed study of neuropsychiatric disorders in a North Swedish population of about 9000 persons. He contended that certain of such disorders are genetic in origin and suggested that *oligophrenia spastica* is a simple recessive. Tage Kemp presented a great deal of data gathered by the University Institute of Human Genetics, Copenhagen, on the "Frequency of Diseases Affected by Heredity in Denmark." Among his conclusions is "that the proportional frequency of endogenous cases for many hereditary lesions is increasing as a result of the progress of civilization." This is food for thought if further studies confirm it. J. V. Neel closed the first session on the topic with a study of "The Population Genetics of Two Inherited Blood Dyscrasias in Man." The conditions studied were thalassemia (Cooley's anemia) and sickle cell disease. Although much research has been expended upon the problems, Dr. Neel found his data insufficient for a firm proof of the genetics of transmission. The two hypotheses he favors are "factor interaction on the part of two independent genes" and "that these two genes are actually allelomorphs." L. H. Snyder summarized the day's program with a discussion of the importance of considering polygenes as well as major genes as the causative agents in many kinds of physiological variations.

The second session devoted to racial traits brought forth three papers on morphological variations among men. W. S. Laughlin discussed "Blood Groups, Morphology and Population Size of the Eskimos" as the first of these. He used blood groups to demonstrate that "Not only are the Eskimos distinguished from the Indians by their blood groups, they are different from all other populations for whom data are available, when MN types and Rh factors are also employed." He pled that whole populations, not lesser breeding isolates, be used when racial studies are under way.

When this is done for the Eskimos it appears that they are different from the American Indian and from the Asiatic Mongoloids, probably being closer to the latter. J. N. Spuhler presented the second paper, "Genetics of Three Normal Morphological Variations." These are:

a) A common character classifiable into two discrete phenotypes following a simple mode of inheritance with full penetrance, e.g., patterns of superficial veins on anterior thorax; b) a common character classifiable into two phenotypes following a simple mode of inheritance on the qualifying assumption of reduced penetrance and variable expression, e.g., absence of the peroneus tertius muscle; c) a common character classifiable into several discrete phenotypes, e.g., number of vallate papillae.

The last paper in this group was "Genetic Analysis of Racial Traits of the Teeth," by G. W. Lasker. In it he reviewed the rather sizable corpus of data devoted to variations in human teeth. In spite of the importance of the subject to the student of fossil man, there has been little or no work done on the genetics of teeth in modern man. Such studies are necessary before the teeth of early homonids can be evaluated properly.

The final section devoted to the genetics of racial traits treated blood groups. R. R. Race, A. E. Mourant, and W. C. Boyd presented papers. The first two detailed evidence based upon the analysis of blood groupings; Boyd discussed the situation more broadly, but certainly he was influenced by his deep knowledge of the genetics of blood groups. Race discussed "Eight Blood Group Systems and their Inheritance": Landsteiner, MNS, P, Rh, Lutheran, Kell, Lewis, and Duffy blood groups. Each of these systems is both serologically and genetically independent of all others. Thus "It is quite possible therefore that in the eight blood groups systems we have markers for eight of the 23 autosomes of man." Mourant's paper on "Blood Groups of the People of the Mediterranean Area" followed. In it he demonstrated the distribution of Landsteiner and Rh groups and of Cooley's anemia. From this he summarized:

Blood group A shows high frequency in European lands bordering the Western Mediterranean and in Greece. Group B has high frequency all around the Eastern Mediterranean. The Sardinians, the Basques and certain Berber tribes in North Africa have a high O frequency. The Rh blood groups of the Mediterranean lands differ considerably from those of Northern Europe. Two main strains can be recognized, typified by the Basques with high cde(r) and the Sardinians with high CDe(R_1). These, together with a North European and a Negro strain, are sufficient to account for the Rh groups of all Mediterranean populations yet examined.

From his accumulated data Mourant postulated certain migrations of the Mediterranean peoples. The final paper of the session was Boyd's "Three General Types of Racial Characteristics," which conform with the three types of characteristics detailed by Spuhler in an earlier paper in the symposium.

The seventh session dealt with "Race Concept and Human Races." Five papers were presented by C. S. Coon, J. B. Birdsell, M. F. A. Montagu, B. Lundman, and J. L. Angel, with L. C. Dunn presiding. Coon discussed the racial implications of certain social traits that vary with the advance and complexity of human societies. As might be expected, this evoked varied and critical discussion. Dr. Coon brought out certain physiological advantages in reproductive capacity held by hunter-fisher folk as opposed to modern societies, the likelihood of reaching reproductive age enjoyed by offspring of modern societies as against the high mortality of offspring of primitive societies, and the ability of the individual in modern society to foster group survival at his own peril. Coon believes that these and similar cultural traits play an important part in the genetics of race. Birdsell's paper, a lengthy one devoted to detailed studies of several inherited characteristics among Australian aborigines, stresses the importance of studies of gene flow. His admirable distribution maps of certain genetic conditions clearly support his contention that the study of Huxlian clines among humans is profitable. His paper is a model that might well be studied with profit by all biogeographers. From my own experience with invertebrates I heartily agree with the sentiment of his concluding sentence, on the urgent need for an adequate space calculus.

The third paper in this series was "A Consideration of the Concept of Race," by Montagu. It is sharply divided into two sections, the concept of race and the mechanism of race formation. At the outset, Dr. Montagu makes clear his stand that only the biological concept of race (subspecies) is valid for anthropology, that the politico-social concept is so confused as to be worthless. His concept of biological race is based upon these three interrelated postulates: (1) Race applies to populations not individuals; (2) race is a statistical concept; (3) race is a comparative or relative term. Since so much confusion exists about race in the lay mind (and in not a few trained minds!), Montagu again suggested that the word be dropped from the professional vocabulary of anthropology and that "ethnic group" be substituted. This section of his paper might well be required reading for all taxonomists, particularly entomologists. The second part of the paper is based upon the postulate that "the original ancestral human popu-

lation was genetically relatively heterogeneous" and split into more or less isolated breeding units. Seven forces worked upon these dispersed groups: natural selection, mutation, isolation, genetic drift, hybridization, sexual selection, and social selection. Each of these was examined by Montagu in the light of our present knowledge of man. In conclusion, he considered man a sympatric polytypic species wherein "the amalgamation of all varieties of man into a single variable population is but a matter of time."

In the concluding papers of the session Lundman presented a series of "Anthropological Maps of the Nordic Countries," and Angel discussed "Population Size and Microevolution in Greece." The first of these relates to the present population and is the sort of presentation possible where many extended population studies have been made. The second shows the variation found in skeletal material over a span of 4500 years. Data are given representing twelve periods well distributed from 2500 B. C. to A. D. 1939.

The over-all topic for the eighth day was "Constitution." S. M. Garn opened the session with "The Constitutional Modification of Mendelian Traits in Man," a rather brief and not too satisfactory statement of the interreactions of genes controlling physique and physiology. The paucity or lack of genetic evidence for either of these human attributes prevented Dr. Garn from making firm statements. Thus his paper suggests lines for future research rather than demonstrated results. C. C. Seltzer's "Constitutional Aspects of Juvenile Delinquency" was based upon a study of 500 male juvenile delinquents and a matched group of non-delinquents. His conclusion, that "Although delinquents are physically different as a group from non-delinquents as a group, there is no implication of fixed criminal anthropological types, inherent criminality or criminal personality," is more or less in line with recent findings by others. W. H. Sheldon concluded this session with "The Somatotype, the Morphophenotype and the Morphogenotype." This stirred up a rather heated and prolonged discussion. The geneticists objected strenuously to the use of "morphophenotype" and "morphogenotype" without any proved genetic basis for their use. What is clear from Dr. Sheldon's *written* word may not have been so clear from his *spoken* word—he used these terms to differentiate in his discussion what is *seen* in a particular physique from what may *cause* the physique. It is possible that, since his unconventional method of attack on the problem of physical types was so successful in evolving the somatotype system, a similar fresh attack on the somatotypes may produce useful clues to the genetics of human physique. It is evident from Dr. Hooton's closing summary that much of Sheldon's paper is not reproduced in the printed version of the symposium.

The concluding session of the symposium was devoted to "Perspectives of Future Research." It is rather fitting that the two papers composing it represent the extremes of the general problem that had been discussed for eight days. Th. Dobzhansky summarized in "Human Diversity and Adaption" what is known of the genetics of man and pointed out a few of the more promising fields for further research into the major problems: mutation and individual variability, intrapopulation polymorphism, neutral traits and genetic drift, selection in race differentiation. Kluckhohn and Griffith discussed the relationship between "Population Genetics and Social Anthropology." After a philosophical introduction they recounted "some portions of the natural history of one small breeding population, the Ramah Navaho," and suggested that "a mathematics either more complicated or possibly far more simple than any yet devised is required to handle the concrete problems in all their empirical detail."

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THE DAY AND THE DATE LINE

WITH the increasing importance during the past few years of events in Japan, China, and Korea, differences in time have been strikingly called to our attention. For example, it is a familiar experience to sit at home on a Sunday evening and hear over the radio about something that happened a short time before—on Monday morning in Tokyo. And although we realize that this has something to do with the international date line, which passes through the western Pacific, some of us are not too sure as to just what this involves.

First of all, our time is based on the turning of the earth from west to east, which makes the sun and the other celestial bodies seem to turn around us from east to west. The day is based on the time it takes for one complete turn, as measured by the average length of time from a moment when the sun crosses the meridian, directly south, until it does so again.

A place south of us obviously is not south of a point farther west so that, when the sun is on our meridian in New York, marking noon, it has not yet reached the meridian of Chicago. There it is eleven o'clock in the morning. The sun is still farther from the meridian at Denver, where it is only 10:00 A.M.

This, of course, is the reason for time zones, and why it is necessary to set one's watch back an hour for each 15 degrees of longitude traveled to the west. Going all the way around the earth in this direction, one would set his watch back an hour 24 times, and would find himself a day behind his friends who had stayed at home—were not something done to correct it.

But something has been done. It was decided by international agreement in 1884 to set the calendar a day ahead at some stage of the westerly trip around the world. The place for this is called the international date line, which approximately follows the meridian of 180° longitude. West of this line it is always the day after the one being marked to the east. Therefore, moving westward, one always shifts forward a day at the line, and he goes back a day on an easterly passage.

If all places on earth had regular time zones, then the date line would be the place where the new day is born. It would begin at noon, Greenwich time (which is 7:00 A.M., Eastern Standard Time, or midnight at the line). Suppose it has been Sunday. Then Monday starts and advances toward the west with the shift of the meridian that marks midnight. After three hours (10:00 A.M., EST) it has reached Japan, and after six and a half hours (1:30 P.M., EST) the new day reaches India. It comes to central Europe eleven hours after it began (6:00 P.M., EST) and to England and western Europe an hour after that.

Seventeen hours after it began at the date line, Monday reaches New York. Five hours after that it reaches the Hawaiian Islands—at 5:00 A.M., EST—and two hours after *that* the meridian marking midnight reaches the date line once more. Tuesday then repeats the process. Thus, General Ridgway is having Monday's luncheon at his headquarters in Tokyo when people in New York are going to bed Sunday night, and residents of Hawaii are enjoying a late Sunday afternoon stroll.

Actually, of course, the day does not advance smoothly around the world, but in 24 principal steps. Each covers an average of 15 degrees of longitude, or one of the time zones. It becomes Monday all at once from Maine to Michigan and an hour later does the same from Kentucky to Texas. There are many variations from strict adherence to the time zones. On an isolated island in the Pacific, for example, there would be little advantage in using the time of the central meridian of the zone it happens to be in, and many such islands make use of their local time as standard.

Honolulu is practically on the border between two zones, so until a few years ago Hawaiian time was five and a half hours behind Eastern Standard. However, increasingly close contact with the mainland, by means of radio programs, air travel, etc., led in 1947 to adoption of 150th meridian time, which is only two hours behind Pacific standard and five behind EST.

In the time zone through which the date line passes—that is, the one with the central meridian of 180° longitude—some curious things happen in regard to the date. Normal standard time here would be just twelve hours different from Greenwich—fast for places west of the date line and slow for those to the east. In other words, it is the same time, but different days. Compared with EST, 180th meridian time would be seven hours slow to the east of the date line, and seventeen hours fast to the west.

The date line, however, does not follow the 180th meridian precisely but has several kinks in it. This is done to prevent the complications that would arise if one group of islands, right on the meridian and under one political administration, had two different days in two different parts of

their territory. One of these kinks, to the west, includes the westernmost of the Aleutians; the other, to the east, takes in many of the islands of the southwest Pacific.

The Chatham Islands, east of New Zealand and under its sovereignty, are included in this region. They observe time that is 12 hours 15 minutes fast of Greenwich. To the north of them are the Tonga Islands, which the noonday sun reaches still earlier, so their time is 12 hours 19 minutes 12 seconds fast of Greenwich. It is said that an even greater difference is arbitrarily observed in Wrangel Island, a Soviet possession just north of the easternmost tip of Siberia. Although on the 180th meridian, this island, according to the best information available, uses time that is 13 hours fast of Greenwich, or 18 hours ahead of New York standard time. In other words, the inhabitants of Wrangel use permanent daylight saving time.

TABLE 1

New York Date	EST	Local Time	Event
Wednesday, March 19	6:00 A.M.	Midnight	March 20 begins at Wrangel
	Midnight	"	March 20 begins at New York
Thursday, March 20	6:00 A.M.	"	March 21 begins at Wrangel
	6:20 A.M.	"	March 20 begins at Niue and Savage
	Midnight	"	March 20 ends at New York, March 21 begins
Friday, March 21	6:00 A.M.	"	March 22 begins at Wrangel
	6:20 A.M.	"	March 20 ends at Niue and Savage

To the east of the date line the slowest time is apparently that used on Niue Island and Savage Island, which is 11 hours 20 minutes behind Greenwich, or 6 hours 20 minutes behind the eastern United States. No place, so far as we know, uses time any slower than this.

All this leads to a curious situation regarding the length of the day. To most people, of course, the day lasts 24 hours, which is perfectly true—for one location. But, one may ask, how long does a single day survive someplace on the earth? If the day starts at the date line, takes 24 hours to encircle the globe as it pushes the previous day out of existence ahead of it, then itself is squeezed into oblivion through another 24 hours as the next day advances, it would last a total of 48 hours. This, however, does not allow for such complications as Wrangel, Tonga, and Savage Islands.

It is at Wrangel that tomorrow began, at 6:00 A.M., EST, this morning. At about 6:40 A.M. it started at Tonga, and at 6:45 at Chatham. At midnight tonight it will reach us eighteen hours after it began, and today will become tomorrow. But tomorrow morning at 6:00 A.M., EST, the day after tomorrow will start at Wrangel, to follow the career of its predecessor.

At 6:00 A.M. tomorrow, however, by New York time, it will still be today in many of the Pacific Islands just east of the date line. Moreover, at the last place to have a particular day, Savage and Niue Islands, it will still be today until 6:20 A.M. tomorrow by EST. Tomorrow will not end there until 6:20 A.M., the day after tomorrow, by New York time. This will be 48 hours 20 minutes after tomorrow began this morning in Wrangel, so the one day will have lasted a total of 48 hours 20 minutes.

Perhaps this would be clearer if tabulated for a particular day, such as that of the vernal equinox, which in 1952 occurred on Thursday, March 20 (Table 1).

This shows, incidentally, that for 20 minutes, from 6:00 A.M. to 6:20 A.M., EST, three different days are simultaneously in existence in some part of the earth. During this period it is tomorrow at Wrangel, yesterday at Niue and Savage, and today in the rest of the world.

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BOOK REVIEWS

FROGS AND SALAMANDERS

Amphibians of Western North America. Robert C. Stebbins. ix + 539 pp. Illus. $7.50. University of California Press, Berkeley and Los Angeles. 1951.

THE herpetological colleagues of R. C. Stebbins were delighted by the brilliance and the novelty of the methods used in his monograph of the salamander genus *Ensatina.* In *Amphibians of Western North America* he has greatly extended the scope of his studies and appeals to a much larger group of readers.

The area taken as "western North America" is that part of the continent north of the Mexican border and west of the eastern borders of New Mexico, Colorado, Wyoming, Montana, and Saskatchewan. The Saskatchewan border is prolonged northward to the Arctic. This area, although allowing for the inclusion of all Rocky Mountain species, involves the inclusion of most of the Great Plains species.

The fauna is almost equally divided between salamanders (22 species, 44 races) and frogs (29 species, 42 races). Since most of the salamanders have neither voices nor larval stages, the treatment of the frogs is considerably the more lengthy.

The species is regarded as the essential biological entity, and the races are properly subordinated to them. Each species is treated under a minimum of four headings: Range, Type Locality, Description, Habitat. After Habitat may follow: Behavior, Voice, Thermal Data, Food, Reproduction, Remarks (or Nomenclatorial History, Biogeographic Considerations), Subspecies. Structure, range, and sometimes habits, are given for genera and families.

There is thus a wealth of information, which is supplemented by a wealth of illustrations, ingeniously selected, and carefully arranged so that similar species, or parts of them, appear juxtaposed and contrasted in the same figure. This is especially notable and helpful in the various keys.

Following the section on the Salientia, the known eggs are illustrated, and then the larvae. A series of habitat illustrations is followed by a set of distribution maps, arranged so that the degree of sympatry or lack of it can be compared for related forms. A glossary is followed by a bibliography. There is an index.

This book is another welcome move away from the meticulous description of the superficial aspects of one or two preserved individuals, and toward the description of all aspects of the species as a group of individuals living in an environment. In cases where there is a difference of opinion, both sides of the case are fairly presented. There is also frank admission of gaps in our present knowledge, and Stebbins has made his book not only a guide to what is known, but an aid in determining what is not known.

I find much to praise and little to criticize. In the preface the "eastern border of Alberta" is said to delimit the area in Canada, but on the accompanying map, and in the distribution maps, it would seem that the eastern border of Saskatchewan was actually used.

On page 226 the genus *Bufo* is said to occur in the Aleutian Islands, but in the range of *Bufo boreas* is said to extend only to southeastern Alaska and is mapped by locality dots only to the base of the Kenai Peninsula.

The author has followed a prevalent custom of providing photographs of habitats. At the risk of voicing heresy, I deplore this custom. It is expensive, and I doubt very much whether it conveys as much information as the written word. One could produce replicas of most of these twenty-three illustrations in eastern Pennsylvania.

Eastern students will be interested in the vast area now occupied by the Eastern bullfrog (which in the Great Valley of California has occupied a region left vacant by the native Western forms), and be amused by the fact that green frogs (*Rana clamitans*) occur in the extreme Northwest, apparently introduced by misidentification.

Eastern students cannot fail to be stimulated by Stebbins' original methods and format, and view the book somewhat enviously and with a faint sense of shame; just as they must also do when they look east at Smith's *Reptiles and Amphibians of the British Isles.*

Dr. Stebbins and his publishers are to be congratulated on this very fine piece of natural history.

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DANGER, LANGUAGE AT WORK

The Art of Clear Thinking. Rudolf Flesch. vii + 212 pp. $2.75. Harper, New York. 1951.

The Art of Eloquence. Theodore R. McKeldin and John C. Krantz, Jr. x + 234 pp. $3.00. Williams & Wilkins, Baltimore. 1952.

RUDOLF FLESCH, who developed a formula for measuring readability in writing, has done his best, in his latest book, to eliminate formal logic as a barrier to clear thinking. This is not inconsistent since he defines formal logic as a set of rules for a yes-no parlor game or "an unnatural way of thinking, a contrived technique of going from unwarranted assumptions to foregone conclusions." His writing formula simply measures general ease of reading, not creative talent.

In prose that certainly meets the requirements of readable writing, Dr. Flesch has simplified much of the current literature and research on how we think. This book is for the general reader—but it may profitably be

studied by specialists, even in the field of logic. The author not only amplifies his points with exceptionally apt illustrations, but he explains in nontechnical language. He does not write down to his reader but he does not assume that the reader knows the recent developments in psychology, "thinking machines," legal procedures, public opinion polling, and semantics.

Flesch sets his own rules for this book in a two-sentence foreword, which reads: "It would be impudent to tell intelligent, grown-up people how to think. All I have tried to do here is to assemble certain known facts about the human mind and put them in plain English."

And he admirably attains his goal.

In discussing examples of logical fallacies, Dr. Flesch has turned, tongue in cheek, to mythical advertisements for "Durtee Soap" and come up with illustrations that are accurate yet amusing to all those not connected with advertising agencies. Illustrating the *argumentum ad ignorantiam* ("the stress upon ignorance," as he explains it) the author writes, "Only Durtee Soap contains the miracle ingredient Lodahocum. If you've never heard of Lodahocum, you ought to be ashamed of yourself."

The author has many kind words to say about science and what he calls in one of his chapter headings, The More or Less Scientific Method. For example, this quotation is taken from that chapter:

> For the layman, the most important thing about science is this: that it isn't a search for truth but a search for error. The scientist lives in a world where truth is unattainable, but where it's always possible to find errors in the long-settled or the obvious. . . . So-called "scientific" books that are supposed to contain final answers are never scientific. Science is forever self-correcting and changing; what is put forth as gospel truth cannot be science.

And, again, this is Flesch's conclusion to his final chapter:

> Yes, clear thinking is rare. To approach it, we need above all that indispensable quality of the scientific spirit —humility. Like the good scientists, we must be ready to sacrifice some of our personality and habits of thought as we face each new problem. For life's problems are always new, and defy all ready-made solutions.
> That's what makes life so interesting.

The book contains a two-page bibliographical "Reading List" and 16 pages of "Notes" citing sources from which Dr. Flesch got his ideas and examples.

In the second of these "how-to-do" books, Governor Theodore R. McKeldin of Maryland and Dr. John C. Krantz, Jr., professor of pharmacology at the University of Maryland School of Medicine, team up to discuss effective public speaking. Both have had extensive training and all the copious illustrations (Commencement Addresses, Speeches of Welcome, Addresses for Raising Money for a Cause, and Inspirational Addresses) are taken exclusively from their own files.

Typical of their approach are these three basic rules taken from the first chapter on You, Too, Can Be a Speaker:

1. Determine that you are going to be a successful public speaker.
2. Know your subject.
3. Assume a successful attitude. . . . Act in a confident manner and you will feel confident.

The book may be helpful to the person who has stage-fright at the thought of getting up to make his first public appearance. It contains a wide range of talks given by the two authors, which may be tailored to fit almost any public occasion.

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WHAT MAKES SAMMY RUN?

The Explanation of Human Behavior. F. V. Smith. ix + 276 pp. $2.75. Constable and Co., London; Macmillan, New York. 1952.

THIS book presents a review of various theories, or systems, of psychology through which the mass and variety of data concerning human behavior have been given meaning. Providing this meaning, via systems, involves a consideration, first of all, of the psychological factors involved in the process of explaining. An explanatory system is an expression of a theoretical bias, of a point of view, a value-system, or a system of choices. Thus, to give some insight into the nature of this explaining behavior, and thereby to make meaningful a consideration of the application of a system in the integration of observed and recorded data, the author begins, in Part I of his book, with consideration of such matters as (1) characteristic features of the human mind in the activity of explaining, (2) the features of the causal relationship and functional dependencies, (3) characteristics of the explaining process, and (4) difficulties encountered with various types of explanation.

This last discussion, one of the most valuable in the book, is concerned with such factors as (1) detection of the contributing relationships among variables, (2) difficulties of establishing functional dependence, (3) difficulties in conceiving the relationships of "prescription" (in human behavior, for instance, one deals more generally with probable, rather than prescribed, consequences), (4) difficulties associated with the presence of teleological features, (5) the problem of intervening variables, (6) problems of cause and effect involving intersystem relationships—i.e., qualitative and quantitative continuity and discontinuity between cause and effect interpretations.

The *raison d'être* for the above is the explanatory systems presented for review and evaluation by the author: the systems of Wm. McDougall, Kurt Lewin, G. W. Allport, J. B. Watson, L. Hull, and E. C. Tolman (in that order). For each system the author attempts to cover such points as the following: What is the man trying to do? What explanatory concepts are basic to his system? What method is employed? How well does the system function and achieve its purpose?

The six systems treated by the author survive his evaluation with varying degrees of success, as indicated in his final chapter. To disclose their relative status, without also presenting the basis for this status (which space does not permit here) would do an injustice. It is relevant, however, to indicate the author's evaluation concerning the systems *in toto*:

> It will be apparent that the nature of the process of human behaviour is such that none of the systems herein examined fully meets the logical and indigenous requirements of an adequate explanation. The chief difficulty is the indication of all of those states which are actually continuous with and causally associated with behaviour and the presence of which may be suspected from general experience, general literature and the present state of psychological theory (p. 263).

He then, after further discussion, closes with these remarks: "The question naturally arises as to whether or not Psychology should be influenced by the patterns of procedure in the physical sciences. . . . It may well be that such procedures are not appropriate to the intrinsic nature of psychological processes." How one is inclined to judge the systems, or the criteria employed for their evaluation, depends perhaps upon a still broader belief concerning "explanation"—faith in man's ability to handle the problem, or the need to call on "extraterritorial" assistance. Many more scientists, versed in the history of science, will continue to bet on man, and show renewed efforts to systematically understand and explain the cause-and-effect relationships of human behavior.

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AT THE ZOO

Wild Animals in Captivity. H. Hediger. Trans. by G. Sircom. xi + 207 pp. Illus. $6.00. Butterworths Scientific Publications, London; Academic Press, New York. 1950.

THE author, a biologist and zoo director of note, has a broad knowledge of wild animals in their native haunts, based on observation and on extensive travel in Central Africa, Australia, New Guinea, and the Pacific Islands, as well as of animals in captivity, having been director of two zoos, including the splendid one at Basel, where he is now in charge. His knowledge gained from practical experience has been augmented by a deep and thorough study of the literature on the subject.

The biology of zoological gardens is a complex subject at best, and Hediger has tried to clarify some of its problems. The history of zoos covers three distinct periods: the first of these is the Age of Cults, such as the bear cult of the Stone Age and animal divinities in ancient Egypt and elsewhere. This resulted in the domestication of some animals, from sacred motives.

The author calls the second the Profane Age, when animals were considered for their usefulness and for entertainment, such as animal fights and hunting.

The third might be called the Scientific Age, in which the interests are systematic and anatomical, physiological and psychological, so that the modern zoo is not merely a place of entertainment but an institution which has since the beginning of the scientific age decisively influenced whole trends in natural history. For instance, in the seventeenth century, F. Redi discovered the fundamental biologic law that only living things can generate living things, by observation of the animals in the Florence menagerie. In the same century, W. Harvey discovered the circulation of the blood through the study of animals in Windsor Park.

With scientific accuracy and in an interesting style, Professor Hediger has answered most of the sensible questions that are asked in zoos. He describes habits of animals in their native state and explains their adaptations when they live in captivity. Lions in the field roam long distances, sometimes covering an area of thirteen square miles, in search of prey, but they can readily adapt themselves to, and take their exercise in, comparatively small quarters.

Housing, feeding, and care, so that an animal may live in conformity with its native habits and instincts, are treated scientifically and psychologically. There is even a chapter on the psychology of zoo visitors.

The text is followed by a splendid bibliography. Altogether, this book is an excellent manual on zoo management, but contains, also, a great deal of interest to zoo visitors and to all interested in animal life.

W. M. MANN

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THE RAINBOW'S FABULOUS END

Nobel: The Man and his Prizes. Edited by the Nobel Foundation. 620 pp. Illus. $6.00. University of Oklahoma Press, Norman. 1951.

WHEN Alfred Nobel died in 1896 and left the fortune he made from high explosives to establish the famous prizes that bear his name, he probably had in mind something rather different from what they have actually become. His will, written in November 1895, provided that the prizes be distributed "to those who, during the preceding year, shall have conferred the greatest benefit on mankind," in the fields of physiology or medicine, physics, chemistry, literature (for the "most outstanding work of an idealistic tendency"), and peace.

It is now of course obvious that no one can tell what discovery of the year immediately past will be of greatest benefit—often decades are required before such value becomes apparent. Accordingly, the original statutes of the Nobel Foundation, as announced in 1900, honored at least the spirit of Nobel's instruction by providing that the phrase "during the preceding year" should be "understood in the sense that the awards shall be made for the most recent achievements in the fields of culture referred to in the will and for older works only if their significance has not become apparent until recently."

The interesting story of Nobel, and the manner in

which his instructions have been followed, making his prizes the most honored of all awards, is told in this book by eight authors who can speak with the greatest authority. They are the chairmen of the committees responsible for the physics, chemistry, and the medicine and physiology prizes (Manne Siegbahn, Arne Westgren, and Göran Liljestrand); the permanent secretary of the Swedish Academy (Anders Osterling), who is also chairman of its Nobel Committee, responsible for the literature prize; and the director of the Norwegian Nobel Institute (August Schou), responsible for the peace prize. Henrik Schück, late president of the Nobel Foundation, wrote the article on Nobel; and the late Ragnar Sohlman, an executor of Nobel's will, wrote the account of the establishment of the foundation. The foundation's present executive director, Nils K. Stahle, has written a survey of its finances and administration.

In the articles on the scientific prizes, which tell in some detail, as based on the official minutes and records, the reasons that led to the various awards, there is an excellent summary of much of the scientific progress during the past half-century, for little outstanding work has been neglected. Similarly the accounts of the literature and peace prizes give a concise history of developments in these fields.

Of particular interest are the full accounts of the negotiations dealing with prizes given to Germans during Hitler's regime, which led to his ire and an edict that his followers could not accept them. Fortunately some of these prize winners finally received their awards. One was Gerhard Domagk, who was given the 1939 prize in medicine for his pioneering work on the sulfonamides. Even though he could not accept them at the time, the medal and certificate had been prepared and held, and he was able to go to Stockholm for the 1947 ceremonies and receive them, somewhat belatedly.

Though issued by an American publisher, the book was printed in Stockholm, and represents a fine example of modern Swedish typography.

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ON THE ROOF OF THE WORLD

Search for the Spiny Babbler: An Adventure in Nepal. S. Dillon Ripley. viii + 301 pp. Illus. \$4.00. Houghton Mifflin, Boston. 1952.

UNDER this cryptic title is described an expedition to Nepal sponsored by the Smithsonian Institution, the National Geographic Society, and Yale University, where the author is an assistant professor of zoology and associate curator of the Peabody Museum of Natural History. Enthusiastic interest in rediscovering rare species of birds collected from 1821 to 1843 by Brian Hodgson, who was sent to Nepal as a clerk by the British East India Company, was the mainspring of the trip. The idea originated when Dr. Ripley, on an ornithological trip in India, had the rare opportunity of spending a month collecting in and around Katmandu, the capital of Nepal. He hopefully made overtures to the maharajah for future more extensive collecting and was not rebuffed.

Nepal, extending five hundred miles east and west, forms the southern slope of the "roof of the world," for the highest peaks of the Himalayas are within its northern borders, and it dips southward one hundred miles to the Indian plains. This dramatic country is closed to Westerners; only two hundred Europeans and two dozen Americans have entered it, and they have virtually followed only the direct route to Katmandu and its environs. Even Hodgson's own collecting was confined to the Katmandu Valley—his 563 species were shot there or were brought to him by Nepali hunters.

The expedition was organized to collect birds and small mammals. A photographer of the National Geographic Society and his assistant joined the zoological staff of four. Arrangements had been made with the government only for the party to go to the capital, a trip involving trains of increasingly narrow gauge, an ancient bus, and ponies to cross the mountains into the Katmandu Valley, where the city presented a curious mixture of the sixteenth and twentieth centuries.

The important permission to visit remote valleys west and east of Nepal was granted. In the west, one of the chief *desiderata,* the spiny babbler, a thrushlike bird belonging to an Old World family, was shot, but the mountain quail, another *rara avis,* was not found. Some 1600 specimens of birds belonging to 331 species and subspecies were secured, and about 200 mammals. Another 50 birds were identified in the field. The author comments repeatedly on the diminution and possible loss of fauna and flora through deforestation and cultivation of the soil.

Amusing stories of the people and of the petty annoyances and difficulties of moving through the country are interwoven with nontechnical descriptions of the birds and other animals, and the terrain, in an easy narrative style. A map showing the routes followed by the expedition is a satisfying addition to this enjoyable account of a little-known country. The photographs, especially a rare view of Mount Everest from the southwest, make one wish that more had been included.

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NORTHEASTERN FLORA

Illustrated Guide to Trees and Shrubs. Arthur Harmount Graves. x + 240 pp. Illus. \$4.40. Author, Wallingford, Conn. 1952.

PROBABLY the plants of no other section of North America are so abundantly represented by check lists, floras, and manuals as those of the Northeastern United States. Woody plant manuals are especially numerous, yet there appears to be a dearth of adequately illustrated available publications featuring both trees and shrubs in a single volume. Dr. Graves' *Illustrated Guide to Trees and Shrubs,* which includes 45 plates and 116 text figures, fulfills this need ad-

mirably. The region specifically covered is New England, New York, New Jersey, Delaware, and Pennsylvania. However, the general utility of the book may well be extended to the North Central and the Lake states. The woody plants featured are those trees and shrubs both wild and cultivated, native and exotic, that are likely to be found in this area. Subalpine and rare shrubs, as well as numerous species of difficult taxonomic groups, such as *Crataegus, Rubus,* and *Amelanchier,* have been omitted.

The general key, based chiefly upon leaves but utilizing also fruits, winter characters, habits, and habitats, facilitates identification of the genera described or figured in the text. Species keys and descriptions accompany the plates and text figures. On the whole the keys appear to be accurate and workable. They are in part polychotomous, however; thus the user must be careful to read all the alternatives (as many as five in some cases) before making a choice. Contrasting legs of the keys are headed by capital letters, and these letters are used repeatedly, often on the same page. This may lead to confusion and may make it difficult to trace the specimen back through the key in case a wrong choice has been made.

The text contains a glossary, an index of scientific and common names, and a list of 52 helpful references. Included, as supplementary material, is a section entitled Short Cuts to Naming. This list of characters, which is arranged in modified key form, serves as an alternate method of procedure and is especially useful in identifying incomplete specimens.

An outstanding feature of the book is the more than 300 pen-and-ink drawings executed by a noted botanical artist, Maud H. Purdy, staff artist emeritus of the Brooklyn Botanic Garden. The detail and accuracy shown in the reproduction of leaves, fruits, and greatly enlarged winter buds are praiseworthy. It is a real pleasure to view page after page of these original and excellent drawings reproduced from copper plates.

The accurate descriptions and pertinent comments throughout the work reflect the intimate knowledge of the subject acquired only by keen observation and long association with plants in the field. More than forty years of teaching experience have gone into the making of this book. The author is to be congratulated on a fine job well done.

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INCREDIBLE RECORD

Magic Oil, Servant of the World. Alfred M. Leeston. 237 pp. $3.75. Juan Pablos Books, Dallas. 1952.

The author of *Magic Oil* is one of a small group of competent observers who, when they looked closely at the American petroleum industry, have seen revealed in the amazing record of its achievement compelling evidence of the supremacy of free enterprise as an agency of social and economic accomplishment.

Two thirds of all the oil the world has consumed in the past has come from the United States. Per unit area of oil-bearing rocks, we have produced ten times as much oil as the rest of the world. Yet the earth's crust, outside the boundaries of the United States, contains far larger quantities, far richer accumulations, of oil than we have possessed.

No one planned this achievement. No one looking into the future even believed it possible. On the contrary, thirty years ago, when past production had amounted to hardly more than 10 per cent of our present total past production, leaders in government, science, and industry alike were already crying out in alarm that our petroleum resources were on the verge of exhaustion. Yet, continuously since that time, we have successfully met the demand for oil in this country—a demand that in the meantime has increased fivefold. And our proved reserves of oil in the ground are now fivefold greater than they were when the dire predictions were made.

All this has come to pass in a field of endeavor that has correctly been designated as a natural monopoly. Nevertheless, as demand mounted and the search for new oil fields became more intense, prices declined and quality of products improved.

The record of the American petroleum industry is incredible and it is unparalleled. What are the factors that made it possible?

Magic Oil sets down this record in concise form, analyzes it, and proposes an answer to the question. Dr. Leeston is an economist and an international lawyer by profession. In his study of the oil industry, he has marshaled an astounding array of facts, opinion, and testimony, which he examines with a discerning eye. He has scanned a tremendous volume of source material, which he quotes with discrimination. His principal interest lies in the production of oil, but he discusses the other branches of the industry—transportation, manufacturing, and marketing—instructively. He covers the natural gas industry briefly but effectively.

Magic Oil is a small, thin book but it is packed with information. Apart from the frontispiece, there are no illustrations. Although the treatment centers around the American industry, its scope is world-wide. It includes brief but informative accounts of the activities of other nations and of the production, transportation, refining, and marketing of oil in other countries, an admirably selected bibliography, and a well-prepared index.

As an example of book making, *Magic Oil* leaves something to be desired. The text is unfinished and sorely needs proofreading. Passages are printed out of place. Page references are conveniently numerous, but with annoying frequency are left blank. There are occasional small errors: "(1 barrel 7–7.5 tons);" Venezuelan oil concessions are shown as aggregating 20 million acres on page 192, as 16 million acres on page 193. The English might be described as "quaint," but it is comprehensible, even readable; and altogether the merits of the book far outweigh its shortcomings.

Wallace E. Pratt

Frijole, Culberson County, Texas

BRIEFLY REVIEWED

A Concise History of Astronomy. Peter Doig. xi + 320 pp. $4.75. Philosophical Library, New York. 1951.

AS EDITOR of the *Journal of the British Astronomical Association,* Mr. Doig is in a particularly fortunate position to be familiar with astronomical developments. The BAA, although including many professional astronomers, is primarily an amateur organization, and therefore its articles are somewhat less abstruse than those in some other astronomical publications.

This is reflected in Doig's book, which is well described by its title. Starting with the oldest astronomy, millennia before the birth of Christ, he brings the story well up into the twentieth century, with its theories of the nuclear origin of stellar energy and of the expansion of the universe. Although the treatment of these and other topics is necessarily brief, the reader is given a start, and the list of further references indicates where he might go for more details.

The book was first issued by an English publisher in 1950, and now becomes available in the United States. Except for a new title page, the American and British editions appear identical.

J. S.

Wild Life in Color. Roger Tory Peterson. 191 pp. Illus. $3.00. Houghton Mifflin, Boston. 1951.

FOR more than a decade, the National Wildlife Federation has been issuing colored picture stamps of natural history subjects to help raise funds for its worthy cause. Now Roger Tory Peterson has gathered more than 450 of these attractive plates and arranged them in book form, accompanied by his explanations and descriptions.

The book may be considered a cross section of the natural history of North America. Birds, mammals, and reptiles, as well as trees and flowers, are considered from different angles. Geographically, north, south, east, and west are treated in special chapters and, ecologically, the inhabitants of town and farm, deciduous woodlands, the North Woods, swamps and marshes, grass country, and the desert, as well as those of the far north and the Arctic, are considered. There is a chapter on extinct species and one on introduced ones.

The writer, with a broad knowledge of natural history and an ability to write clearly and pleasantly, has produced an unusual and worth-while book of interest to anyone who likes natural history.

W. M. M.

History of Science and Technology in the 16th and 17th Centuries. A. Wolf (Rev. ed. by Douglas McKie.) xxiii + 692 pp. Illus. $7.00. Macmillan, New York. 1951.

IT WAS in 1935 that the late Professor Wolf's *History of Science and Technology in the 16th and 17th Centuries* first appeared, to be followed in 1939 by a similar volume dealing with the eighteenth century. Both works were well received and have become standard references for their respective periods. Psychology, philosophy, and the social sciences were covered, along with astronomy, chemistry, physics, biology, medicine, and various aspects of engineering.

The "new" edition of the first of these books follows the original very closely, even the pagination being the same, although a note indicates that "a number of errors have been corrected and the bibliographies extended by Dr. D. McKie, Reader in the History of Science, University of London."

J. S.

NEW BOOKS

Modern Magnetism. (3rd rev. ed.) L. F. Bates xii + 506 pp. 30*s*. Cambridge University Press, London and New York. 1951.

The Theory of Electromagnetic Waves. A symposium held under the auspices of the Washington Square College of Arts and Science and the Institute for Mathematics and Mechanics of New York University and the Geophysical Research Directorate of the Air Force Cambridge Research Laboratories, June 6–8, 1950. viii + 393 pp. $6.50 Interscience, New York. 1951.

Anatomy of the Chordates. Charles K. Weichert. vii + 921 pp. Illus. $8.00. McGraw-Hill, New York. 1951.

Nutrition and Climatic Stress with Particular Reference to Man. H. H. Mitchell and Marjorie Edman. xii + 234 pp. Illus. $6.75. Thomas, Springfield, Ill. 1951.

Economic Botany: A Textbook of Useful Plants and Plant Products. (2nd ed.) Albert F. Hill. xii + 560 pp. Illus. $7.00. McGraw-Hill, New York. 1952.

Antibiotic Therapy. Henry Welch and Charles N. Lewis. xiv + 562 pp. $10.00. Arundel, Washington, D. C. 1951.

Liver Disease. A Ciba Foundation Symposium. Sheila Sherlock and G. E. W. Wolstenholme, Eds. xiii + 249 pp. Illus. $5.00. Blakiston, Philadelphia. 1951.

Liver Injury. Transactions of the Tenth Conference May 21–22, 1951, New York. F. W. Hoffbauer, Ed. 320 pp. Illus. $3.75. Josiah Macy, Jr. Foundation, New York. 1951.

Man and his Biological World. (Rev. ed.) Frank Covert Jean *et al.* viii + 631 pp. Illus. $5.00. Ginn, Boston. 1952.

The Porcelain Jacket Crown: A Manual Stressing the Preparation of Normal and Abnormal Teeth. S. Charles Brecker. 263 pp. Illus. $8.00. Mosby, St. Louis. 1951.

A Handbook of Space Flight: Technical Information in Tabular Form. Wayne Proell and Norman J. Bowman. 185 pp. (No price given.) Perastadion, Chicago. 1950.

Quantum Theory of Matter. John C. Slater. xiv + 528 pp. $7.50. McGraw-Hill, New York. 1951.

Television Engineering. (2nd ed.) Donald G. Fink. xiv + 721 pp. Illus. $8.50. McGraw-Hill, New York. 1952.

Graphic Arts in Engineering Computation. Randolph P. Hoelscher, Joseph Norman Arnold, and Stanley H. Pierce. viii + 197 pp. Illus. $4.50. McGraw-Hill, New York. 1952.

The Handbook of Private Schools. (33rd ed.) E. Porter Sargent. An Annual Descriptive Survey of Independent Education. 988 pp. (No price given.) Author, Boston. 1952.

Nuttall's Travels into the Old Northwest. An unpublished 1810 Diary. Jeannette E. Graustein, Ed. 88 pp. + 10 pl. $3.00. Chronica Botanica. Waltham, Mass.; Stechert-Hafner, New York. 1950/51.

LETTERS

THE EMPEROR MARCHES ON

EDITORS' NOTE: Fifteen months after the appearance of "The Emperor's New Clothes, or *Prius Dementat*" (THE SCIENTIFIC MONTHLY, **72**, 32 [1951]), the controversy and correspondence it stirred up continue in undiminished vigor, vehemence, and volume. Excerpts from letters received in February and early March may be of interest, but the anonymity of the writers will be preserved.

"But the charge of bias rests upon . . . substantial grounds. Many educators . . . were amazed and dismayed to find that an editorial board would allow the official organ of the society (AAAS) to be used to attack a portion of its membership. No act of your board since the publication of Fuller's patently unscientific pronouncement has indicated its intention to become unbiased either by publishing equally venomous attacks against all other groups in the membership of the association or by apologizing for its previous action. I shall await with interest your publication of an attack on the chemists."

"If your judgment of the worth of a contribution is based on the number of inquiries you receive, I have a few suggestions. Get a physicist to write on the weaknesses of training in psychology, say, as compared with that in dianetics; or a geologist to ridicule the doctors for opposing socialized medicine. Or you might find an up-and-coming young chemist to tell what is the matter with the law schools which are responsible for all the crime that is going on.

"You seem as yet to be quite unaware that it is the responsibility of a journal published by the AAAS (The 'S' stands for Science) to give its readers dependable knowledge. After all, THE SCIENTIFIC MONTHLY is presumably not a journal of opinion, biased or otherwise. . . . Fuller apparently has no conception of sampling techniques, statistical differences, and cause-and-effect relationships. . . . Others as thoughtless as he find his display of ignorance satisfying."

"Congratulations for the clear and truthful article by Harry J. Fuller in THE SCIENTIFIC MONTHLY of Jan. 1951!"

"Professor Harry Fuller has referred my question to you: do you have available any more reprints of Fuller's 'The Emperor's New Clothes'? . . . I should like very much to have ten copies. I should be happy to pay any cost. I want to perform some of my own distribution of Mr. Fuller's doctrines."

"The Department of Education of the Graduate School of Yale University is deeply concerned with contemporary criticisms of the public schools. . . . We are now exploring the preparation of an anthology of 'best' criticisms, together with the 'best' answers to these criticisms. We are especially interested in statements, pro and con, that have recently appeared in the better general periodicals, as well as articles more or less regularly published in the national professional education journals. . . . Fuller, H. J., 'The Emperor's New Clothes,' 72: 32, January, 1951. Williams, Simon, and James D. Lauritz, 'Scientists and Education,' 72: 282–283, May, 1951."

THE MESH IS TOO FINE

IT SEEMS to me that the appended editorial from the Minneapolis *Morning Tribune* may be worth reprinting.

E. C. STAKMAN

University of Minnesota, St. Paul

The McCarran internal security act has fulfilled some of the worst fears of its critics since it was passed in September 1950. In the *Tribune* this week, Victor Cohn reported on one phase of its fulfillment. Inquires made in Washington as to its operation reveal that the act tends to keep reputable foreign scientists from this country.

To the extent that this is so, there is an impeding of the free flow of scientific information from abroad. The United States is setting up barriers to distinguished visitors who might make important contributions to its fund of knowledge. This loss is particularly regrettable in the field of basic theory, where we have lagged behind Europe.

There can be no quarreling with the McCarran act's objective of increased internal security. But its provisions dealing with the admission of foreigners to this country have too fine a mesh. They screen out not only Communists and other genuine security risks but also persons of good reputation and solid achievement.

These persons cannot even get visas to attend a scientific conference. They are tossed into the same basket with all sorts of undesirables whose presence in this country might, in fact, endanger our security. Cohn reports that visa decisions under the McCarran act are made in an atmosphere of fear. Refusal is the easiest course. The State Department does not want to take chances on stirring up an "incident." So renowned scientists find our doors closed to them. We are narrowing the flow of scientific knowledge which common sense tells us should be widened.

The registration provisions of the McCarran act have also run into trouble. Not a single Communist-action or Communist-front organization has yet conformed to those provisions. The Commies are not cooperating, and laying the groundwork for prosecution is a long and technical job. It was predicted when the law was passed that registration provisions would be extremely difficult to enforce. Experience thus far has shown this to be true.

Congress needs to take another look at the McCarran act. Even Senator McCarran would be willing to make it easier for some foreigners to get visas by relaxing the restraints a little. No change should be made which would in any way jeopardize national security, but it is obvious that the law can be modified in some respects without such penalizing of our basic interests. It is particularly important that we put a longer latchstring out for foreign scientists. The present one is far too difficult to reach.

SOUTHWESTERN DIVISION · AAAS
Twenty-Eighth Annual Meeting
Jointly with the
Colorado-Wyoming Academy of Science
May 1–4 at the University of Colorado, Boulder

Boulder is 25 minutes from Denver on this turnpike

ORGANIZED in 1920 to serve the steadily increasing number of scientists resident in the Southwest, the Southwestern Division of the AAAS now has nearly 1000 members. Its territory extends from Sonora and Arizona, through New Mexico, to the 100th meridian in West Texas, thence northward to Colorado. Its annual meeting is normally held during the first week in May. In 1950 its members assembled at Flagstaff, Arizona, near the western limit of its range, and they concluded their deliberations in a snowstorm on the South Rim of Grand Canyon. In 1951 the meeting was held at El Paso, close to the southeastern corner of divisional territorial limits, and this year the group moves north to Boulder, Colorado.

In fact, the division will reach beyond the limits of its domain, for the Boulder meeting will be co-sponsored by the Colorado-Wyoming Academy of Science, a AAAS affiliate. Headed by Earl A. Engle, chemist at the University of Denver, the academy carries on its activities in nine separate sections, publishing its proceedings and some of the papers presented at its meetings in its own journal. With its 400 members joining forces with the Southwestern Division, the Boulder meeting will be larger than usual; but, even so, the anticipated attendance of 500, on the campus of the University of Colorado, should furnish a pleasing contrast to those who find the December meetings of the Association too large.

There is nothing exclusive about the meeting, and scientists from every part of the country will be welcome. For years the Southwestern states have furnished summer laboratories for many of the biologists, geologists, anthropologists, and astronomers of the nation, and the Boulder meeting will be a forum of current research in these fields. Mining and agriculture have provided raw materials for a regional chemical industry, and war brought many physicists to some of the governments' most critical and classified installations, thus rounding out the Southwest as a center of active research in every field of science and applied science. Visiting scientists from outside the Division will thus find much more in the meeting than a hearty welcome.

For information
On the meeting, write Frank E. E. Germann, University of Colorado
On Boulder and its environs, write the Secretary, Boulder Chamber of Commerce